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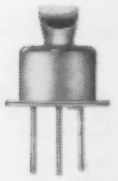

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Members x 2

Professors sometimes remark that the university would be an ideal place if it were not for the students. But no one ever makes such quips about an association. They may complain about the officers, or the policies, the publications, or the meetings; they may dislike some of their fellow members; but everyone agrees that an association *is* the members who comprise it. Within rough limits, the rate of growth of membership is a function of the usefulness of the association, and, again within rough limits, an increase in membership allows the association to be of greater service to its membership, for the cost of operation increases less rapidly than does the number of members.

AAAS membership has shown marked growth since its founding 110 years ago, and had a big spurt in the years surrounding the centennial celebration in 1948. A well organized membership drive increased the total from 29,000 in 1946 to 45,000 in 1949. Then came a slack period; from 1950 through 1954 the annual increases were small, and totaled only 4000 in the five-year period. Since 1954, however, there has been an upturn, with a gain of 7000 in the past three years.

The Board of Directors has decided that a substantial further increase is in the interest of all members and that this year, when the Association completes its 110th year, is a good time to make a special effort to increase the membership. Accordingly, each member was recently mailed a letter requesting him to invite one or more of his nonmember colleagues to join the Association. The every-member-get-another-member idea is more than a membership-drive gimmick; scientists who are already members are the best judges of who among their colleagues would find AAAS membership most profitable, and experience shows that their nominees are likely to join.

The members of any association must decide what the admission requirements will be. Some societies set quite specific requirements in terms of education or experience; others welcome anyone who is sincerely interested in the association's objectives and activities. The AAAS followed the British Association in adopting the latter policy. Thus anyone who is sincerely interested in the advancement of science and who wishes to receive *Science* is eligible for membership.

There are, of course, other reasons than the receipt of *Science* for joining the AAAS, yet that is the most frequent and tangible advantage of membership. It is an advantage that is also enjoyed by a considerable number of nonmembers, for sample surveys of readers have consistently shown that the number of readers is about twice as large as the number of subscribers. These nonmember readers are a good potential source of additional members, for their active interest in one aspect of the Association's program has already been demonstrated.

The letter that the Board of Directors sent to all members not only invited each to nominate a new member but also urged him to encourage his nominee to accept the invitation to join. Early returns indicate a large response, but there are still many members to be heard from. This editorial is a reminder that the invitation was seriously meant; we hope that the members will nominate a very substantial number of new members. This editorial is also an invitation to readers who are not members. An application form is printed on page 1450. Nonmembers who are interested in the objectives and activities of the AAAS are invited to use it.—D.W.

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SCIENCE

RRENT PROBLEMS IN RESEARCH

Near Eastern Prehistory

The swing from food-collecting cultures to village-farming communities is still imperfectly understood.

Robert J. Braidwood

The Near East (or Middle East—I ve no preference) is traditionally ken to mean the area that stretches om the Libyan flanks of Egypt to in- ude all of southwestern Asia as far as e rim of the Baluchi Hills, which over- ok the Indus Valley. The Indus itself, rts of Transcaspian Turkestan and ranscaucasia, and even Greece and the udan might also be included, but this not usual. The geographic core of the ea is the drainage basin of the twin vers, the Euphrates and Tigris, and the ighlands and plateaus which immedi- ely flank this drainage basin (Fig. 1). n this sense, the Nile and Indus basins e on the western and eastern boun- aries of the core area, as do the Medi- erranean, Black, Caspian, and Red seas nd the Indian Ocean (*1*).

he Area and Its General Problems

Since all human prehistory is re- ricted to the Quaternary period, it is fficient to say that the Pleistocene ysiographic history of the area has een essentially one of superficial ero- on and deposition, sometimes on a rge scale (*2*). The area shows traces such world-wide climatically deter- ined features as high marine and river rraces and localized glaciation in the gher mountains, but the over-all struc- tural geography and the positions of the major land masses and seas were essentially set in pre-Pleistocene times. It now appears that extreme climatic change during the late glacial to early postglacial range of time was not *the* important factor in the appearance of plant and animal domestication. C. E. P. Brooks' much quoted (*3*) "propinquity theory," which attempted to explain the appearance of food production through the concentration of men, plants, and animals in oases and river valleys as the Atlantic rain winds withdrew northward at the end of the last glaciation, is no longer tenable (*4*).

Unfortunately the geochronological details of Pleistocene events in the Near East may not yet be directly equated with those of western Europe, save in a most general way. This lack of intercontinental geochronological precision allows differences of opinion among prehistorians about how this or that range of Pleistocene artifacts in the Near East may be related to more or less similar types in Europe. Sometimes these disagreements have bearing on the construction of grand syntheses of culture-historical evolution (*5*).

For the prehistorian—for any culture-historian for that matter—the area was the scene of three great culture-historical events.

1) The earliest appearance (on present evidence, if we take the more probable geochronological long view) of the blade-tool tradition. This relatively sophisticated set of habits in the preparation of long parallel-sided flint tools seems to have been roughly coincident, in Europe, with the appearance of anatomically modern men, about 40,000 years ago. The Palestinian ("nonclassic" or "sapiensized") Neanderthals may be regarded as ancestral to modern men (*6*), and the blade-tools make a tentative appearance in the Syrian and Palestinian stratigraphy even earlier than do these unspecialized physical types. It is not impossible, therefore, that the general Near Eastern area was the focus of differentiation and eventual spread of anatomically modern man and of his earliest characteristic habits in the preparation of flint tools.

2) The earliest appearance of the settled village-farming community, based on small-grain agriculture and animal domestication, about 10,000 years ago. The word *agriculture* is here used in a more restricted sense than that given it by Sauer (*7*). This was Childe's (*8*) "food-producing revolution" par excellence, and its consequences were momentous. It is probably very difficult for us now to conceptualize fully (or to exaggerate) the consequences of the first appearance of effective food production. The whole range of human existence, from the biological (including diet, demography, disease, and so on) through the cultural (social organization, politics, religion, esthetics, and so forth) bands of the spectrum took on completely new dimensions.

3) The earliest appearance of urban *civilization*, first in alluvial Mesopotamia, about 5500 years ago, and only slightly later in Egypt. This is usually categorized archeologically by certain reclaimable artifactual criteria (*9*) such as cities, monumentality in art and architecture, public works, and writing, but the general social and cultural implications of the achievement were even broader (*10*). In fact, there is no general agreement with Childe in considering this step a further "revolution" on technological-economic grounds alone (*11*; *12*, p. 72). Civilization appeared as a special intensification of cultural activity which effective food production made possible, but it was not necessarily

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the predetermined consequence of food production.

The subject matter of this article does not include the third event, which needs delineation of its own, requiring far more space than is available here. Nor shall I examine here the other experiments in the achievement of effective food production and of civilization which occurred, at slightly later times, in other parts of the world.

It will quickly become apparent that the reclamation and interpretation of the culture-historical evidence for Near Eastern prehistory is only in its infancy. The broad outline and the major problems are beginning to come into focus, and the research tools are being sharpened, but we still have a very long way to go. In the range of time we deal with here, each of the levels of culture involved required a very intimate balance with its environment. Superficially, it does not seem necessary for our own culture to maintain such a balance, due to vastly more sophisticated means of production, transportation, and distribution. But the expert in prehistoric archeology faces the duty of reclamation and interpretation in two realms: culture history and natural history. He will have been trained, more or less well, to cope with and be thoughtful about the evidence for culture history, and this in itself is a full-time job and more. But archeologists' excursions into natural history have usually ended in disaster; evidently competence in the biological and earth sciences also demands a full-time commitment! (*13*).

During our last (1954–55) field season in Iraqi Kurdistan, for work on the problem of the appearance of the settled village-farming community, we were enabled (*14*) to take out a skeleton team of natural scientists: a botanist, a geologist, a radiocarbon and ceramic-soils technician, and a zoologist. What we learned together, in daily communication in the area itself, about the reconstruction of an ancient environment doubtless marks a new departure in the study of prehistory. It is probably also worth saying that such teamwork between archeologists and natural scientists is not without contemporary importance. Both the Israeli and Iraqi governments are utilizing such teams in gaining knowledge about how ancient irrigation and land-usage patterns functioned (and eventually failed to function) in making their plans for modern land-reclamation projects (*15*). What is important for our present purposes, however, is that the archeologist (both in the range of prehistory and of conventional ancient history, for that matter) is faced with problems which have dimensions that go into sciences far beyond his competence. A joint attack on these problems, with at least some field participation and the establishment of easy communication with interested natural scientists, does pay off handsomely.

Pleistocene Prehistory of the Near East

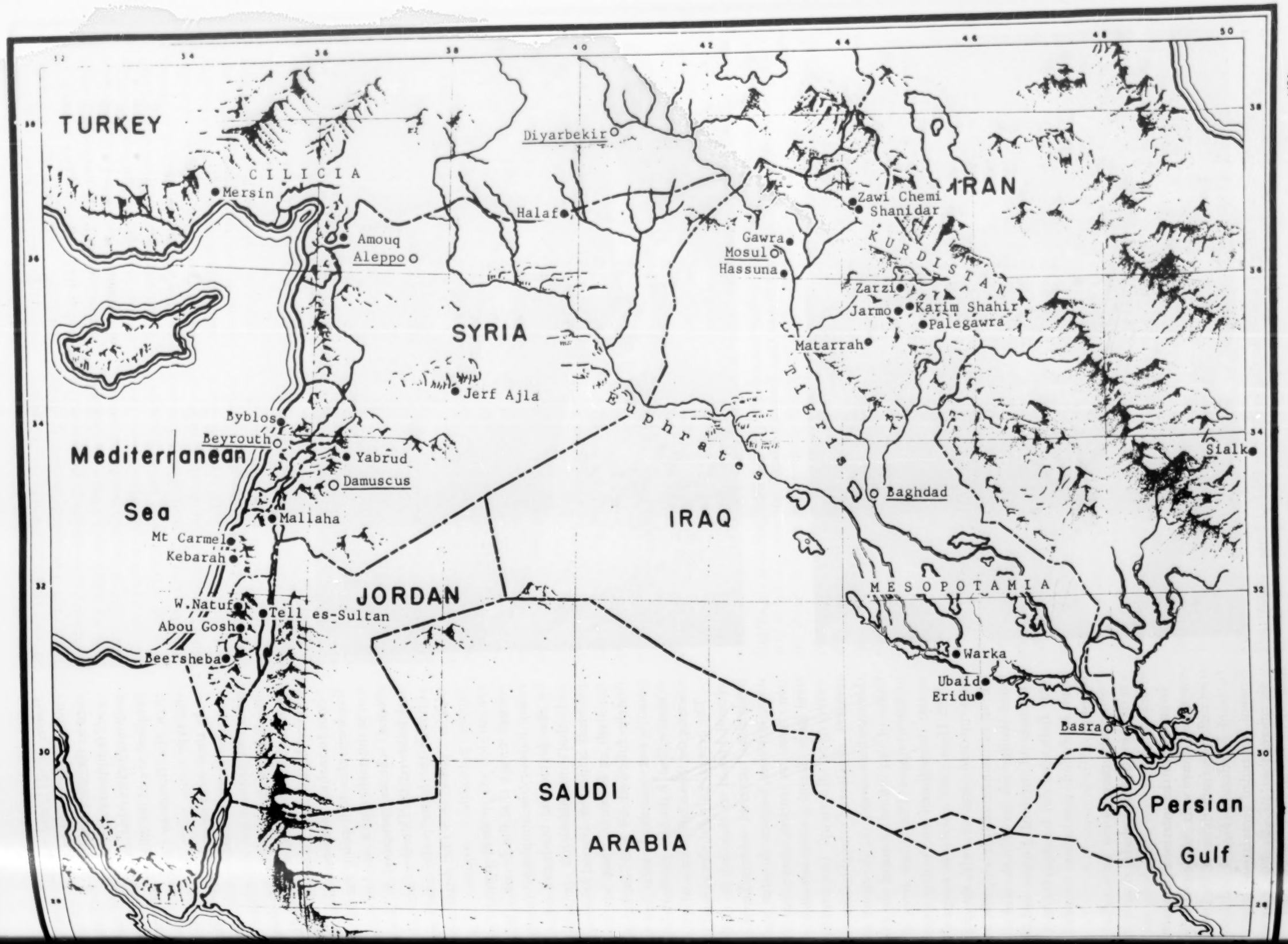
The basis for subdivision of earlier Pleistocene times is somewhat confused, but a working definition might be that the lower Pleistocene proceeds from the end of the Villafranchian fauna to the end of the Mindel glaciation. The middle Pleistocene runs thence to the end of the Riss glaciation, and the upper Pleistocene runs from the Riss/Würm interglacial to about 10,000 years ago. Fleisch (*16*) assigns a few rolled flint tools to the +45-meter marine terraces near Beyrouth, Lebanon (which some authorities take to be late lower Pleistocene), but a general lower Pleistocene occupation of the Near Eastern area is not yet evidenced. Even middle Pleistocene flint-tool occurrences, again on marine terraces of the east Mediterranean littoral and on the highest Nile terraces yet examined, have only geological (not archeological in the sense of "living site") context at best. There is little question but that men, who prepared their flint tools according to the persisting habits of both the core-biface and flake-tool traditions (*17*, *18*), had already arrived in the Near East by middle Pleistocene times, but we have, so far, little knowledge of their culture history. Really early traces of Pleistocene men, such as have been found in southern and northwestern Africa, have not yet been noted in the Near East.

In the geochronological long view (see above, and *5*), the archeological sequences in several caves near the east Mediterranean littoral began to be deposited early in upper Pleistocene times, if not with the recession of the Riss glaciation itself. An excellent sequence from fossil springs in the Kharga Oasis west of the Nile (*19*) parallels the littoral sequence in its earlier ranges, and the tools from the Nile terrace fit this same picture in a general way. On both sides of Suez there were fluctuations in utilization of various types of tools: coarse flake-tool industries (Tayacian), developed core-bifaces (Acheulean and Micoquian), and developed flake tools (Levalloiso-Mousterian, and so on). Clark Howell's (*20*) detailed synthesis of these developments will soon be available.

Three remarkable things appear in our present knowledge of the earlier portion of upper Pleistocene times in the Near East. The first is the tentative occurrence of the blade-tool tradition, in the Tabun cave on Mount Carmel, in contexts which include Acheulean core-biface tools; blades also appear in the Yabrud cave near Damascus soon thereafter. The second is the appearance, in the just-subsequent Levalloiso-Mousterian levels on Mount Carmel and in nearby caves, of fossil men who show a trend toward anatomically modern morphology (*6*). The third is the apparent long persistence, in Egypt and its environs, of the Levalloiso-Mousterian industries, after—at the end of the earlier subphase of the upper Pleistocene—the blade-tool tradition had taken over in southwestern Asia. If the geochronology is as we expect, the early appearance in southwestern Asia of the blade-tools and of human beings with anatomical tendencies toward modern man (at a time when "classic" Neanderthal man was flourishing in western Europe) makes this area a focus of some interest. There is not, of course, complete agreement that either the blade tools or anatomically modern men did first appear in the area (*6*). The long persistence and diminution in size of Levalloiso-Mousterian tools in Egypt remain inexplicable in the light of our knowledge of southwest Asia, but this trend parallels what happened in the rest of Africa. There is some promise that work in caves in Libya (*21*) may help elucidate the Egyptian situation, which is still poorly known for later upper Pleistocene times.

Traces of the earlier aspects of the upper Pleistocene are now being recovered in Iran and Turkey and in the Tigris-Euphrates basin. A typologically quite

Fig. 1. Map of the core area of the Near East in prehistoric times, showing the positions of sites for which radiocarbon determinations are available and of certain other key sites. Modern cities are underlined and designated with an open circle (for example, ○ *Baghdad*). The "hilly flanks" natural habitat zone follows an arc from Kurdistan to north of the city of Diyarbekir, to Cilicia, and thence down the Syro-Palestinian littoral.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early open site, Barda Balka in Iraqi Kurdistan, worked by the Iraq-Jarmo project staff for the Iraqi Directorate General of Antiquities (*22, 23*), yielded tools of the earliest of the standardized traditions—the pebble tools—along with upper Acheulean core-bifaces and a flake-tool facies. Howe compares the industry, on typological grounds, with so-called "lower paleolithic" occurrences in both northwest Africa and in the Punjab, and Wright suggests that its geochronological position is probably contemporaneous with the onset of the Würm glaciation. Core-bifaces and earlier aspects of the Levalloiso-Mousterian flake tools are reasonably common surface finds in the core of southwestern Asia, as they are on its Mediterranean littoral. The cave sequences in the interior, with the exceptions of the core-bifaces at the bottom of Jerf Ajla, near Palmyra (*24*), begin with a developed Levalloiso-Mousterian industry. This industry, first discovered by Garrod in Iraqi Kurdistan in 1928, has since been tested in several caves by the Iraq-Jarmo project (Howe, *23, 25*), and Solecki (*26*) has recovered the remains of several fossil men in the same horizon at the Shanidar cave. It appears to at least some human paleontologists that the physical types involved are of the Mount Carmel rather than the European "classic" Neanderthal type.

Fig. 2. Site of the Zarzi cave (late or food-collecting era of the food-gathering stage) in the intermontane valley south of the Dukan Gap, Iraqi Kurdistan.

Fig. 3. The Palegawra cave (late or food-collecting era of the food-gathering stage) in the Bazian intermontane valley, Iraqi Kurdistan.

As time went on in the upper Pleistocene, blade tools began to make a more persistent appearance in the higher levels of the caves along the east Mediterranean littoral. The now developed Levalloiso-Mousterian flake-tool industry began to include blade-tools and a peculiar long, thin flint point, and this horizon is the earliest of a six-phase developmental scheme, proposed by Neuville and followed by Dorothy Garrod (*27*), although the latter prefaces the scheme with a "phase 0." The details of this "upper paleolithic" sequence are not critical for our present purposes; it is enough to say that the Levalloiso-Mousterian industry is completely superseded by the developing blade-tool industries and that microbladelets (microliths) presently appeared. Garrod believes that the sixth or Kebaran phase was immediately followed by the Natufian; this point is not completely clear from the evidence, but I do feel justified in considering the Natufian postglacial in time.

The general cultural picture is still not so well known as that for the roughly equivalent range in western Europe. It does, however, suggest the same transition from an earlier, more "natural" food gathering to a more intensified collecting type of activity. Two interesting remarks of Garrod's might summarize this six-phase range; (i) the climate and fauna of the littoral changed very little in latest upper Pleistocene times, and in the immediately succeeding range—which Haas (*28*) assesses as being essentially modern; and (ii) with the speeding up of change and development, detailed similarities between the blade-tool sequences of western Europe and the Near East need not be expected, as cultural evolution now starts to outstrip diffusion (*29*).

Strangely, there is not yet a radiocarbon date for this late glacial range in the littoral.

Even within the interior of southwestern Asia, there were blade-tool industries differing from those of the littoral. In Iraqi Kurdistan (Figs. 2 and 3) the Zarzian "extended Gravettian" industry, with microliths (*23, 27, 30*), is known now to be prefaced at Shanidar by the

Fig. 4. The Karim Shahir open site (era of incipient cultivation) in the intermontane valley of Chemchemal, Iraqi Kurdistan, above white bluff, center.

ier Baradostian industry (*26, 30*). e Baradostian has two radiocarbon tes (*31*): 29,500 ± 1500 years (sample 178), and older than 34,000 years mple W-180). The base of the Zar- in at Shanidar is dated at 12,000 ± 400 rs, or about 10,000 B.C. (sample 179). A new date of about 8650 B.C. mple W-667) for the upper part of Zarzian at Shanidar is now an- nced (*31a*). Howe finds it increas- gly impressive, as more caves are tested Kurdistan, that no post-Zarzian mate- ls have appeared in caves (save the dments left by occasional transients). idently the transition to year-around en-settlement living immediately fol- wed the Zarzian range. Reed's (*23*) eliminary examination of the faunal mains from several Zarzian horizons s convinced him that an essentially odern climate had already been estab- hed.

On the eastern flank of the core area, Afghanistan, the Kara Kamar cave s yielded blade tools and steep scrap- s with radiocarbon dates comparable those of the Baradostian (*24*), but a velopmental sequence in the area is t yet available. To the west, in the hyan cave in Haua Fteah (*21*), on the her hand, blade tools appear to have rived late; this seems to be in keep- g with the curious flake-tool conserva- sm noticed earlier for Egypt.

There are doubtless at least several sconformities (for which industries ve yet to be discovered and interca- ted) in the archeological sequences of e Lebano-Palestinian littoral and of raqi Kurdistan. These are, so far, the ly areas known in any detail. While ere is a gratifying increase in the at- ntion now being given to the climatic nd environmental history of the late leistocene to early postglacial time nge in the area (H. E. Wright, Jr., *23*;), it appears increasingly certain that uch more effort will have to be given the reconstruction of the natural his- ry of the region. It might be said in is connection that a liberalization—in e interest of prehistory, of the national ntiquities laws of some of the countries the Near East would stimulate more eld research. Many of these laws had their purpose the very justifiable pre- ntion of exploitation, by foreigners, of pectacular sites of the historic range, ut the laws have been applied to the etriment of prehistorians and their col- agues in the natural sciences (who ed to study materials in their home boratories) (*33*). But enough is al- ready known of parts of the area to suggest that, at least in its upper Pleistocene range, it will yet yield answers to many of the more meaningful questions about how man became what he was ten thousand years ago.

Postglacial Prehistory

There is increasing agreement among some geologists (*34, 35*) that the late glacial to early postglacial time boundary, in what is now the North Temperate Zone, is to be set at about 10,000 years ago, or 8000 B.C. (*36*). There is also an increasing number of radioactive carbon dates for sites in the era of the settled village-farming communities in the Near East which show that this era must already have been established by about 9000 years ago, or 7000 B.C. Between the earliest village sites known to us and such terminal Pleistocene industries as the Kebaran and the Zarzian, mentioned above, there are clear hints of a range of materials probably best conceived of as the traces of incipient cultivators (*37*). If Solecki's single radiocarbon date for the beginning of the Zarzian is essentially correct (sample W-179, 12,000 ± 400 years before the present) and some time is allowed for the flourishing of this industry [as the newly announced date of sample W-667 suggests (*31a*)] and of its possible Kebaran equivalent, then the sites (Fig. 4) of the incipient cultivators probably were in use about 10,000 years ago, or 8000 B.C.—at the onset of early post-glacial times. Within a thousand years, this experimental cultivation and—in the Kurdish area, at least—year-around life in the open were succeeded by the settled village-farming community.

The chronology suggested in the above sketch—the correctness of which is not yet guaranteed—could not have been given prior to January of 1958 and depends primarily on a new but modest-sized cluster of radiocarbon dates, from samples in northern Iraq and from two sites on the littoral, counted by Meyer Rubin of the U.S. Geological Survey in Washington (*38*). Unfortunately, all the problems of the "geobiochemical" contamination of radiocarbon samples, before they reach the counter, beset the use of this and several other series of radiocarbon dates from the Near East. In Figs. 5 and 6 are plotted the available radiocarbon dates for the Near East [save for samples W-667 and W-681 (*31a*)], each date being shown as a time-bar to indicate the counter's plus-minus factor. It is clear that at the present moment (and this will be true until many more samples are counted, from many more different sites), the available fabric of radiocarbon dates can give us no more than a *general* indication of the late prehistoric time ranges of the area. This will throw us back primarily upon our old-style typological assessments of the comparative archeological stratigraphy (*39*) of the various sites in the area. To these assessments we may then add our own judgments of the dating probabili-

ties based on the general pattern of the radiocarbon dating fabric. For the Near East, at least, the cutting edge of radiocarbon dating as a research tool is still blunt because of our difficulties with the "geobiochemical" contamination factor. Understanding of this contamination factor will demand competences in a middle ground lying between archeology and nuclear physics, which badly need to be developed.

The chronological sketch at the beginning of this section was made by selecting a cluster of three radiocarbon dates (samples W-607, -651, -652) of about 8500 years ago, or 6500 B.C., as the probable true general date (out of a series of 11) for the early village site of Jarmo (Figs. 7 and 8) in Iraqi Kurdistan (*17, 23*). Jarmo was a single-phase ma... festation which cannot have had a ti... duration of more than a few hund... years (*40*). The next phase of the e... village-farming community era, in ter... of comparative archeological strati... phy, as seen at Matarrah (sample W-6... and Hassuna (sample W-660) in the ...

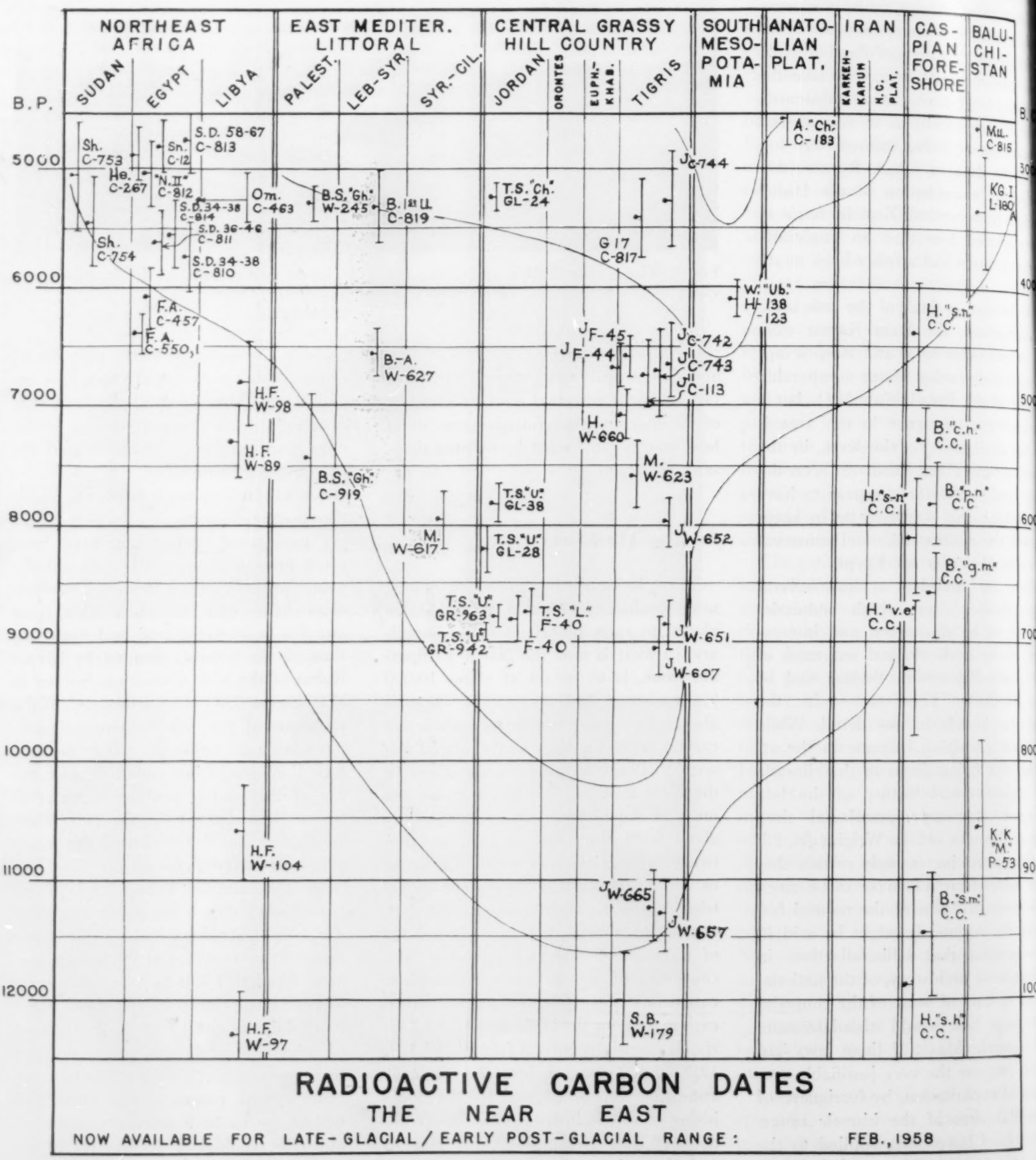


Fig. 5. Summary of the positions, in time and general geographical region, of the now available radioactive carbon dates in the Ne... East, for the range from circa 5000 to 12,000 years ago. The curves suggest levels or eras of food-getting practices, as shown in Fig. ... Most of the archeological sites involved are shown in a previously published table (*53*). See also Solecki and Rubin (*31a*). A key ... these dates appears in Table 1.

er Tigris piedmont and at Mersin (sam-
le W-617) on the Cilician coast of Tur-
ey, seems to cluster at between 500 and
000 years later, say at about 5750 B.C.
Since each of the pertinent phases, on
he sites mentioned, will probably have
had durations of several hundred years, no essential gap between the Jarmo phase and the Matarrah-Hassuna and Mersin phase need be postulated. The group of five Jarmo dates (samples C-113, -742, -743, and F-44, -45) will not work if the Matarrah, Hassuna, and Mersin dates are correct, since Jarmo clearly precedes the pertinent basal materials of these sites in terms of comparative archeological stratigraphy and has several categories of technological

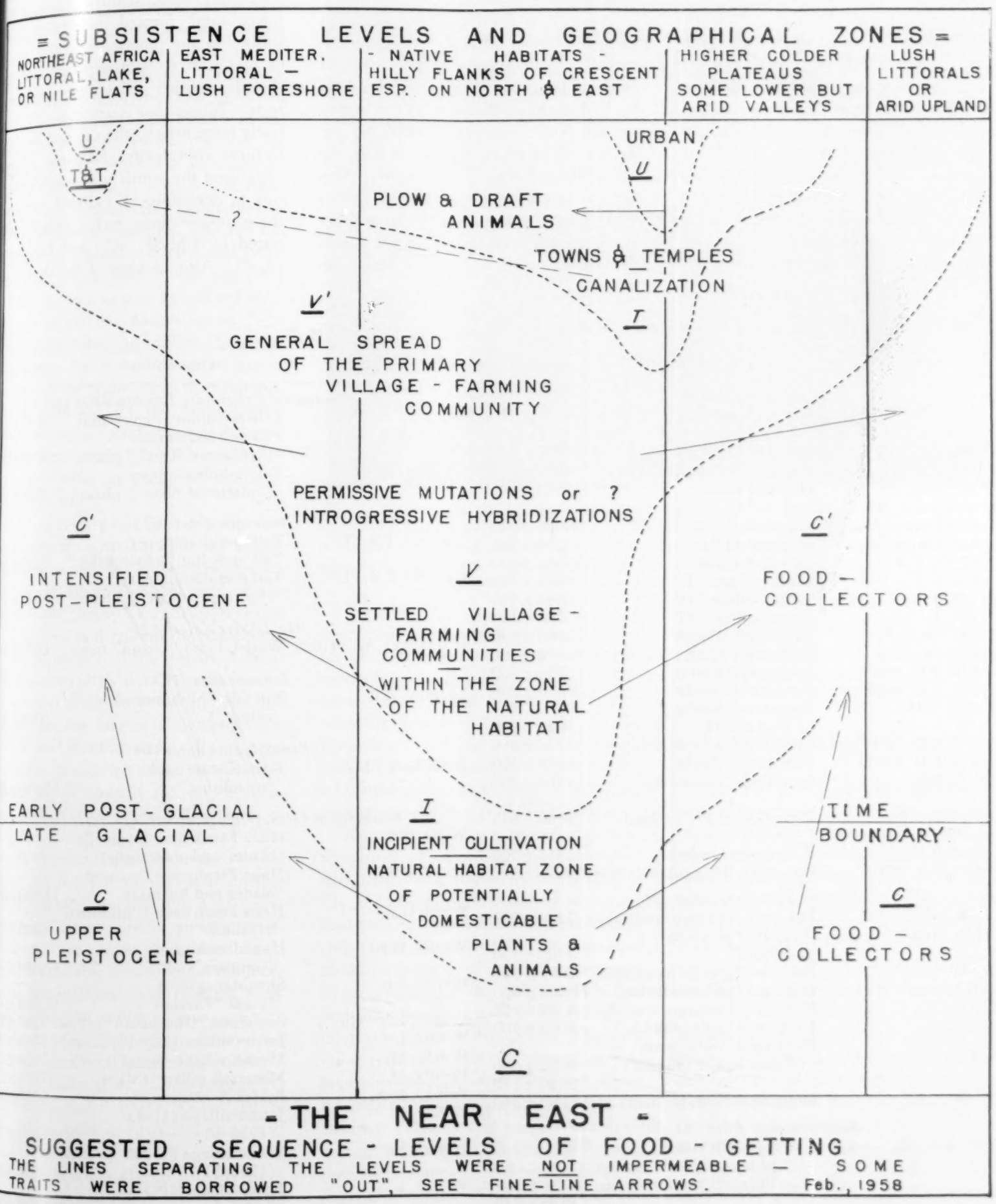


Fig. 6. Summary of the levels or eras of food-getting practices in the Near East with respect to the times and geographical regions shown in Fig. 5: *C*, food-collecting; *C'*, intensified food-collecting; *I*, incipient cultivation; *V*, village-farming communities; *V'*, intensified village-farming communities; *T* + *T*, towns and temples; *U*, urban communities.

descendants in Matarrah and Hassuna. The two earliest Jarmo dates (samples W-657, -665), of over 11,000 years ago, are simply not conceivable in terms of comparative archeological stratigraphy as we now understand it. Jarmo must lie near, but not at, the very beginning of the era of village-farming communities; in my judgment this beginning should be put at about 7000 B.C.

It should be made clear that Jarmo is *not* conceived of as *the* spot where the village-farming community level of existence came into being—we do not even believe that there ever was one single such spot—but only that Jarmo represents the earliest example of settled village life which the accident of its prior discovery has allowed us to use as a basis for description (*41*). To my mind, however, it is not an accident that Jarmo was found in the hilly-flanks zone of the "fertile crescent." This zone of upper piedmont and intermontane valleys, stretching at least from Syro-Cilicia into Iran, flanking the Taurus-Zagros arc and still receiving ample rainfall, appears increasingly to have been *the* natural habitat of the potentially domesticable plants and animals (*4, 11, 23*).

There is a complication. Early in 1956, the excavators of Tell es-Sultan (usually taken to have been the site of Joshua's Jericho), in the Dead Sea Valley in Jordan, published a pair of radiocarbon dates (samples GL-28, -38) for the second phase above base in that site (*42*). More recently, two further dates for the same level (samples GR-942, -963) have become available, as well as a pair of dates for the first phase which parallels the latter ones (*43*). From the point of view of comparative archeological stratigraphy, the two basal phases of Tell es-Sultan are enigmatic, and there is now word (*44*) that the "first" phase may in fact have been preceded by some simpler materials. There is clear evidence of considerable architectural complexity in the basal layer, which includes thick stone-founded fortifications, with a tower, and formed mud-brick house walls, but the remainder of the catalog of materials is relatively primitive and includes neither pottery nor metal objects. Taking her cue from the relatively large area of the site and its architectural complexity, Kathleen Kenyon (*45*) used the words *urban* and *civilization* in describing its cultural level, and these implications were strongly contested by Childe (*46*) and Braidwood (*4, 47*). Also, in view of the then avail-

Table 1. Radiocarbon determinations used in Fig. 5.

Sample	Description	Age (yr)
	Chicago dates (59)	
C-12, Sn.	Sneferu tomb	4802 ± 210 (av.)
C-113, J.	Jarmo village (I-7)	6707 ± 320
C-183, A. "Ch."	Alishar "chalcolithic"	4519 ± 250
C-267, He.	Hemaka tomb	4883 ± 200 (av.)
C-457, F.A.	Fayum A village	6095 ± 250
C-463, Om.	el-Omari village	5256 ± 230
C-550/1, F.A.	Fayum A village	6391 ± 180
C-742, J.	Jarmo village (I-7)	6606 ± 330
C-743, J.	Jarmo village (II-5)	6695 ± 360
C-744, J.	Jarmo village (II-2)	5266 ± 450
C-753, Sh.	Shaheinab village	5060 ± 450
C-754, Sh.	Shaheinab village	5446 ± 380
C-810, S.D. 34-38	Predynastic tombs	5744 ± 300
C-811, S.D. 36-46	Predynastic tombs	5619 ± 280
C-812, N.II	Predynastic tombs ("Nagada II")	5020 ± 290
C-813, S.D. 58-67	Predynastic (?) tombs	4720 ± 310
C-814, S.D. 34-38	Predynastic tombs	5577 ± 300
C-315, Mu.	Mundigak "bronze age"	4580 ± 200 (av.)
C-817, G-17	Tepe Gawra 17 +, Ubaid	5400 ± 325 (av.)
C-819, B. 1st U.	Byblos, "first urban"	5317 ± 300
C-919, B.S. "Gh."	Beersheba "Ghassulian" village	7420 ± 520
	Caspian Foreshore dates (24)	
CC-B. "c.n."	Belt cave "ceramic neolithic"	7280 ± 260
CC-B. "p.n."	Belt cave "preceramic neolithic"	7790 ± 330
CC-B. "g.m."	Belt cave "gazelle mesolithic"	8570 ± 380
CC-B. "s.m."	Belt cave "seal mesolithic"	11,480 ± 550
CC-H. "s.n."	Hotu cave "software neolithic"	6385 ± 425
CC-H."s-n."	Hotu cave "sub-neolithic"	8070 ± 500
CC-H. "v.e."	Hotu cave "vole-eaters" (3 skeletons, 2 samples)	9190 ± 590 9220 ± 570
CC-H. "s.h."	Hotu cave "seal-hunters"	11,860 ± 840
	Davy-Faraday dates (43, 60)	
F-40, T.S. "L."	Tell es-Sultan lower ("hog-backed brick") phase, two different pretreatments	8725 ± 210 8805 ± 210
F-44, J.	Jarmo village (II-5)	6650 ± 170
F-45, J.	Jarmo village (I-8)	6570 ± 165
	Geochronological Laboratory London dates (42)	
GL-24, T.S. "Ch."	Tell es-Sultan "chalcolithic"	5210 ± 110
GL-28, T.S. "U"	Tell es-Sultan upper ("plastered floor") phase	8200 ± 200
GL-38, T.S. "U"	Tell es-Sultan upper ("plastered floor") phase	7800 ± 160
	Groningen dates (61)	
GR-942, T.S. "U"	Tell es-Sultan upper Tell es-Sultan "chalcolithic"	5210 ± 110
GR-963, T.S. "U"	Tell es-Sultan upper ("plastered floor") phase	8785 ± 100
	Heidelberg dates (62)	
H-138/123, W. "Ub."	Warka, basal "Ubaid" town	6070 ± 160
	Lamont dates (63)	
L-180A, K.G.I.	Kili Ghul Mohammed village, I	5300 ± 500
	Pennsylvania dates (64)	
P-53, K.K. "M."	Kara Kamar cave "mesolithic"	10,580 ± 720
	Washington (U.S. Geological Survey) dates (65)	
W-89, H.F.	Haua Fteah cave (evolved blades and microliths)	7300 ± 300
W-97, H.F.	Haua Fteah cave (evolved blades and burins)	12,300 ± 350
W-98, H.F.	Haua Fteah cave ("primitive 'Neolithic' ")	6800 ± 350
W-104, H.F.	Haua Fteah cave (compare W-97)	10,600 ± 400
W-179, S.B.	Shanidar cave, B (basal "Zarzian")	12,000 ± 400
W-245, B.S. "Gh."	Beersheba "Ghassulian" village	5280 ± 150
W-607, J.	Jarmo village (PQ-14, 2.5 m)	9040 ± 250
W-617, M.	Mersin village (basal layer)	7950 ± 250
W-623, M.	Matarrah village (VI-4)	7570 ± 250
W-627, B-A.	Byblos A village	6550 ± 200
W-651, J.	Jarmo village (II-4) (1950-51)	8830 ± 200
W-652, J.	Jarmo village (I-7a) (1950-51)	7950 ± 200
W-657, J.	Jarmo village (PQ-14, 2.25 m)	11,240 ± 300
W-660, H.	Hassuna village (5th level)	7040 ± 200
W-665, J.	Jarmo village (N-18, 2.0 m)	11,200 ± 200

able radiocarbon dates for other roughly comparable materials in the Near East, which were all considerably later, Kenyon was forced to see the Tell es-Sultan material as something which developed without respect to the chronological and developmental framework of the area in which it lay (*48*).

With earlier radiocarbon dates now available elsewhere, this view is no longer necessary. My own tendency, in assessing the dichotomous complexity and primitiveness of the Tell es-Sultan catalog, along with the peculiarity of the ecological niche in which the site lay (some 900 feet below sea level, in an arid valley), is still to suspect that there is some "geobiochemical" contamination in the radiocarbon samples. The site lies in an area of tectonic activity, and faults have been noted, both in the site (*49*) and near it (*50*), as well as upward seepage of radioinactive natural gases (*51*). The contamination possibility, however, clearly calls for competences in assessment which the archeologist does not possess. In addition, on the archeological side, there would certainly not be general agreement with Kenyon's reading of the comparative archeological stratigraphy of her site.

The controversy has been of some culture-historical importance for the reason that Kenyon and her colleagues (see *4*) have raised again the old issue of the "propinquity theory" for an oasis origin of agriculture and animal domestication. In spite of its arid, below-sea-level situation, Tell es-Sultan does lie adjacent to an excellent fresh-water spring. But the evidence for the origins of domestication —while still limited and badly in need of further bolstering—has increasingly pointed rather toward the upper piedmont and intermontane valley zone of the "hilly flanks of the crescent." This not only appears to have been the natural habitat for the potentially domesticable wild plants and animals but also seems to have had no important climatic variation since later upper Pleistocene times. While to take this view may be to ignore certain minor depositional features, especially in the first millennium B.C., which may have been climatically determined (*52*), one does feel justified in making a general assessment of the environmental situation of some 10,000 years ago in terms of the present situation. In fact, it is not clear that, in the core area of the Near East, the late glacial to early postglacial time boundary was at all an "event" in climatic or environmental terms. Some allowance must naturally be made for the loss of vegetation through overgrazing and charcoal burning, and for the extinction of certain wild animals (hunting from Ford model T's finished off the onager about 1928!). Our reconstruction here is founded on the proposition that the available evidence does locate the natural habitat in the hilly flanks zone, and that domestication took place within this natural habitat.

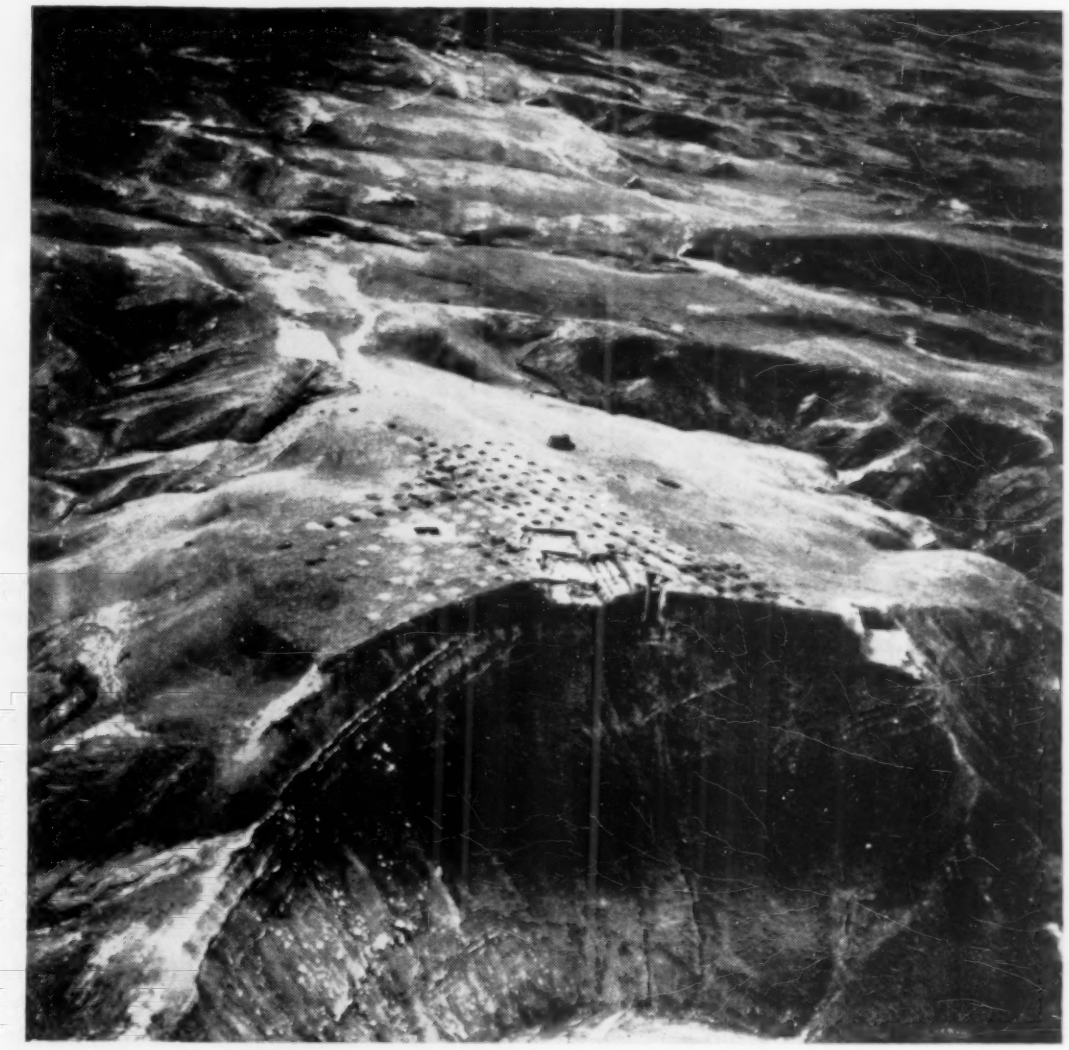


Fig. 7. Air view of the village-farming community cite of Jarmo, in the intermontane valley of Chemchemal, Iraqi Kurdistan. The base exposed in the cut at the extreme right is virgin soil; the grid squares in the center were for the purpose of exploring the village plan in the uppermost levels. [Courtesy of Iraq Petroleum Co., Ltd.]

There are, of course, more excavated sites upon which to base our reconstruction than those which are indicated in Fig. 5 (*53*). But there are by no means enough. Our knowledge of the potentially rich (from the point of view of natural habitat) districts of Iran, Turkey, and Syria is almost, if not completely, blank, and little has been done in this range of interest in Egypt in some years. This situation reflects in part the prevailing unfavorable circumstances discussed above for making an up-to-date prehistoric excavation (Fig. 9) and in part the great cost of mounting a well-rounded expedition (with a proper staff complement of natural scientists). Hence the reconstruction may be, to a degree, an artifact that reflects the incompleteness of the record. A fairly recent assessment of much of the area is available (*37*), and it will therefore not be necessary to name too many of the sites involved.

Incipient Cultivation

Figure 6, which was developed as an overlay on Fig. 5, indicates how the principle of sloping horizons (*12*, p. 34) for the successive eras must be involved in thinking about the subregions of the Near East. I have suggested above the general picture given by the archeological materials for the range of Pleistocene times in the Near East. The means of obtaining food during the whole range was through gathering or collection; the range is usually referred to as the food-gathering stage. If the stage had substages or eras, the last of these was one of more intensive collection—a more intimate "living into" a given environment. We noted that the Kebaran of Palestine and the Zarzian of Iraqi Kurdistan appear to have terminated this era, sometime after 10,000 B.C. [The newly announced date of the upper part of the Shanidar Zarzian is about 8650 B.C. (*31a*).] The data

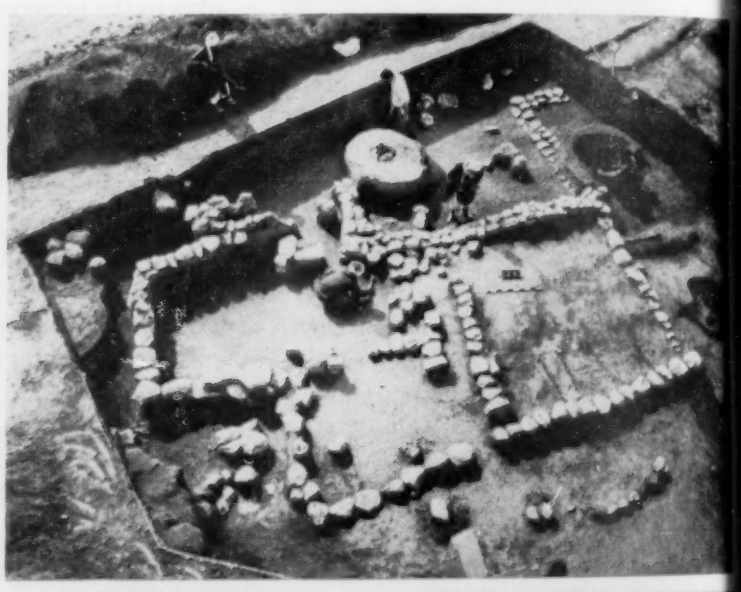
in Fig. 6 suggest, however, that food collection was continued in areas adjacent to the central core of the Near East, and certain materials in caves on the Caspian and Libyan foreshores suggest even further intensifications in collecting activities. In connection with these peripheral areas, the notion of a "mesolithic" stage has been advanced, as in the case of northwestern Europe, to describe archeological materials showing cultural readaptations—still on a food-collecting level—to the post-glacial environment. The notion will gain validity only if a significant environmental change can be shown to have occurred.

It appears increasingly doubtful, however, whether this or any other meaningful concept of "mesolithic-ness" can be applied to the core area. In Iraqi Kurdistan, the next materials (following the Zarzian), from Karim Shahir and comparable sites, which are simple open-air establishments, suggest an incipience of cultivation. One of these open sites, Solecki's Zawi Chemi (*26*), now has a newly announced radiocarbon date of about 8900 B.C. (*31a*, *41*). In Palestine, these are paralleled by the Natufian materials, still primarily in caves but perhaps slightly more convincing as evidence of an era of incipient cultivation. An important Natufian open-air site has just been announced for northern Palestine by Jean Perrot (*54*). This era prefaces the swing from the food-gathering to the food-producing stage. Its catalog includes some suggestion of animal domestication, some authorities claiming domestication of the dog. There are flint sickles for reaping, crude milling stones for grinding seeds, and celts; the latter may have been used as either hoes or axes, or as both. Further delineation of this era is very badly needed, and since the era was one of transition and, doubtless, of making-do with some old tool types, it will be an exceedingly difficult one to substantiate fully. The era is still characterized by flint blade tools and microliths. The probability is that the natural scientists will do better here than the conventional archeologists.

Village-Farming Communities

Next, in the core area, comes the first phase of fully settled village sites, of which Jarmo is simply the earliest example which happens so far to have been found. In the next phase of the village-farming community era, which rather quickly succeeds the Jarmo phase, there are at least five regionally different village assemblages (catalogs of artifactual materials): those of Hassunan type in the upper Tigris piedmont, those of the Amouq A-Mersin type of Syro-Cilicia, those of the third (?) Tell es-Sultan-Abou Gosh type in Inland Palestine, those of the Fayum A type in Egypt, and those of the Sialk I type in northern Iran (*11*, *37*). Unless the radiocarbon dates on the Fayum A of Egypt (samples C-457, -550/1) are wrong—and more samples should be counted—the principle of the sloping horizon is clearly involved. This, of course, has a bearing on the actual chronological position of the Tell es-Sultan materials.

The earliest of the village-farming communities appear to have clustered still within the natural habitat zone of the upper piedmont and intermontane valleys of the "crescent," where the wild wheats, barley, sheep, goats, pigs, cattle, and some kind of equid were all at home in nature. It has been suggested that the development was bound to this zone until permissive mutations (*55*), or introgressive hybridization (*56*), operated, especially on the plants, to allow the domes-



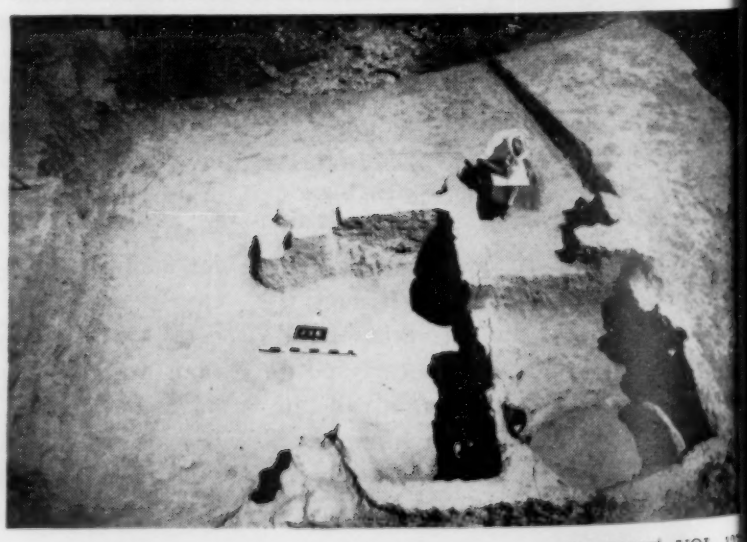


Fig. 8. (Top, left) Partial plan of a mud-walled house in the fifth level in the village site exposed at Jarmo. The white streaks on the room floors are the traces of reeds. (Top, right) Stone foundations of a house in the second level exposed at Jarmo. (Bottom, right) Incomplete plan of a mud-walled house in the sixth level exposed. In this level and at deeper levels portable pottery vessels were not in evidence.

Fig. 9. (Left) Konservator Hans Helbaek of the Danish National Museum, Abullah Said Osman, field superintendent of the Oriental Institute's Iraq-Jarmo project, and Mrs. Robert Braidwood examine a wheat field in the Chemchemal valley. (Right) The "division" of antiquities excavated at the village site of Jarmo. H. E. Dr. Naji al-Asil, director general of antiquities for the Iraqi Government, is in charge.

icates to be removed from their natural area. The curve in Fig. 6 is inflected to suggest more general spread after this had taken place (*57*).

One consequence of this spread was the diffusion of the wheat-barley-sheep-goat-cattle complex, and much of the generalized cultural know-how which had developed with it, to the boundaries of the Near East and far beyond, wherever the environmental situation allowed such spread. We have hints, through radiocarbon dating, that the new way of life had extended well up the Danube Valley by about 4000 B.C. (*46*) and that by 2500 B.C. it had pretty well covered Europe. It also went eastward; wheat, at least, was being grown in China by at least 1500 B.C., although it does not appear to have been the earliest domesticated plant there. A different consequence of the spread from the hilly-flanks zone of the natural habitat—given the mutations or hybridizations—was the apparent "fingering" movement of early farmers down the mud flats of the Tigris and Euphrates into classic southern Mesopotamia (*11*). This probably took place toward the end of the Hassuna phase or early in the succeeding Halaf phase. It is our suggestion that the principles of canalization were learned on these mud flats; canalization made the occupation of classic southern Mesopotamia by farmers feasible. The data in Fig. 6 suggest that a new era arose on this basis in southern Mesopotamia, and one radiocarbon date (sample H-138/123) indicates that this era was well under way by 4000 B.C. This was an era which is archeologically manifested by town-sized settlements, temple structures of some degree of monumentality, metallurgy as a specialized craft, and evidently (since they are already present at the beginning of the next era) the use of draft animals and the plow. Even in the first or Ubaidian phase of this era of towns, the strength of the new cultural potential of southern Mesopotamia is suggested by the *oikoumenē* of the spread of its painted pottery style—from the Mediterranean coast to the rim of the Anatolian plateau to the uplands of Iran (*17*).

This is the place to end our survey; the next era is that of the appearance of urban civilization in southern Mesopotamia, about 3500 B.C., followed by the beginning of the Egyptian dynasties around 3000 B.C., and by that time prehistory per se is theoretically ended in the Near East.

Conclusions

In summary, it needs to be repeated once more that what is offered here is only one prehistorian's interpretation of very incomplete evidence. For late upper Pleistocene times especially, much more must be learned of the environments which were available, of the human physical types (only one juvenile example and various fragmentary bits exist), and of the different cultural levels. Only snatches of evidence are now available for the era of incipient cultivation, which prefaced the great swing from the food-gathering to the food-producing stage, and very sophisticated environmental reconstructions will be necessary before the cultural achievements of this era can gain meaning. The same holds particularly for the earlier phases of the era of the settled-village–farming community. In reconstructing the general culture history of the Near East, for late glacial to early postglacial times, the concept of sloping horizons appears to be a useful one. It also appears that the zone of the natural habitat may have been a focus of "nuclearity," and that some eras and phases of cultural development may have been manifested there but not elsewhere (*58*).

It must be obvious how much the prehistoric archeologist needs the aid of his interested colleagues in the natural sciences. First and foremost, however, the prehistorian's business is with men—with the anthropology of extinct cultures. He needs to discover all he can about the plants and animals that lived with the men, but the plants and animals did not domesticate themselves. Men domesticated them. The prehistorian is very much aware of the innumerable "how" and "why" questions which still confront him. In the Near East, it is simply a matter of his requiring much more information from the good earth, and some help in interpreting it.

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36. Some authorities [for example, E. Antevs, *J. Geol.* 65, 129 (1957)] interpret the evidence as calling for an earlier late glacial–early postglacial time boundary, and Flint (*34*, p. 283) considers the difficulties involved if the time boundary is taken to be a unitary chronological "event" of world-wide validity.
37. R. J. Braidwood and L. Braidwood, *J. World History* (*Cahiers hist. mondiale*) 1, 278 (1953).
38. M. Rubin, personal communication.
39. Such typological assessments are based on the principle of assessing relative age by means of the relative technological complexity of items in the catalogs of several different archeological sites or phases [see G. Clark, *Archaeology and Society* (Harvard Univ. Press, Cambridge, Mass., 1957), p. 151]. The more restricted the region, the better the principle usually works, and it works best of all if "trade objects" link one phase to another. The principle need not imply physical (geological) stratigraphy at all, although sometimes this is also available for parts of the sequence.
40. Because of the internal consistency of its catalog of materials, from top to bottom, Jarmo can only be assessed as a "one-period" site. It seems inconceivable to me that its duration can have been over 500 years at most, and I tend to believe, in fact, that it was probably less. Thus the scatter of Jarmo dates, from C-744 (5266 ± 450 years before the present) to W-657 (11,240 ± 300 years before the present), is archeologically quite unrealistic. This seems to have no relation either to the counters [compare Beersheba in Palestine, where the Chicago counter's date (C-919) ran early and the Washington counter's date (W-245) ran late for the same horizon] or to the way the samples were collected from Jarmo (for example, W-651 was of the same batch of samples, collected in the same way, as the C-742, -743 cluster). My own conclusion is that "geobiochemical" contamination *in situ* must have something to do with the 6000-year scatter of Jarmo dates, and M. Rubin writes that he wholeheartedly concurs.
41. The finding of a new site with Jarmo type material at its base, superimposed by Hassuna-Samarran phase material, has recently been reported from a valley adjacent to that of Jarmo [see H. Ingholt, *Sumer* 13, 214 (1957)]. I believe that Solecki's [illegible] typologically earlier open-air site, Zawi [illegible]emi, is of the preceding era of incipient c[illegible]vation and is to be considered as contemporary with Karim Shahir and several other Iraqi Jarmo project sites (*23*).
42. F. E. Zeuner, *Antiquity* [illegible] 195 (1956).
43. K. Kenyon, *Palestine Exploration Quart.* [illegible] 101 (1957).
44. ———, personal communication from Miss Kenyon.
45. ———, *Antiquity* 30, 184 [illegible]956).
46. V. G. Childe, *ibid.* 31, 36 [illegible]957).
47. Miss Kenyon is due an apology from me, and this is freely given. Basing my opinion on the only cluster of radioactive carbon dates from Jarmo available at that time (C-113, -742, -743), I strongly favored a "short" chronology [*Antiquity* 31, 73 (1957)]. The newer dates (see above) for Jarmo and other sites make a general "short" chronology untenable.
48. K. Kenyon, *Antiquity* 31, 82 (1957).
49. J. Garstang, *The Story of Jericho* (Hodder and Stoughton, London, 1940).
50. F. E. Zeuner, *Geol. Rundschau* 45, 2 (1955).
51. M. Wheeler, *Walls of Jericho* (Chatto and Windus, London, 1956), p. 117.
52. H. E. Wright, Jr., *Bull. Am. School Oriental Research* 128, 11 (1952).
53. Data in Fig. 5 are restricted to the available occurrences which now have radiocarbon dates; compare Figs. 5 and 6 with Table 1 and reference *37*.
54. J. Perrot, *Antiquity and Survival*, 2, 91 (1957).
55. H. Helbaek [see *23* and also *Ann. Rept. Inst. Archaeol. Univ. London* 9, 44 (1953)].
56. J. R. Harlan, personal communication; see also *Brookhaven Symposia in Biol. No.* 9 (1956), p. 191.
57. Obviously, the character of the curves in Fig. 6 depends, among other things, on the way the geographical columns (compare Fig. 5) are arranged. Thus, the sharp rise, followed by inflection, of the right side of the village-farming community curve depends on placing the "south Mesopotamia" column next to the "Tigris" column of Fig. 5. Early villages have not appeared in the rainless alluvium of southern Mesopotamia.
58. G. R. Willey and P. Phillips, *Am. Anthropologist* 57, 723 (1955); in an earlier version of their useful book (see *12*) they used the idea of a "preformative stage" in categorizing certain New World materials. They have now eliminated the "preformative," in part—if I understand them correctly (*12*, p. 105)—because of "the difficulty of finding criteria that would hold for all major areas of New World archeology." I think they may have been overly timid and that some "stages," eras, or phases may not have been affairs of universal validity but may rather have been manifestations of a given environment alone.
59. W. F. Libby, *Radiocarbon Dating* (Univ. of Chicago Press, Chicago, Ill., ed. 2, 1955).
60. From F. E. Zeuner, in correspondence.
61. H. deVries, *Science*, in press.
62. K. O. Münnich, *ibid.* 126, 194 (1957).
63. W. S. Broecker, J. L. Kulp, C. S. Tucek, *ibid.* 124, 154 (1956).
64. C. S. Coon and E. K. Ralph, *ibid.* 122, 92[illegible] (1955).
65. H. E. Suess, *ibid.* 120, 467 (1954); M. Rubin and H. E. Suess, *ibid.* 121, 481 (1955); M. Rubin and H. E. Suess, *ibid.* 123, 442 (1956); M. Rubin, personal communication.

New Reference Nuclide

The use of ^{12}C as the basis for a unified scale of nuclidic masses and atomic weights is proposed.

T. P. Kohman, J. H. E. Mattauch, A. H. Wapstra

There exist at present three scales of atomic masses or weights: (i) the absolute scale based on the gram, (ii) that defined by taking the mass of one atom of the nuclide ^{16}O equal to 16 units (the "physical scale" of "atomic masses" or "nuclidic masses"), and (iii) that taking the average atomic mass of the isotope mixture of "natural" oxygen as 16 units (the "chemical scale" of "atomic weights"). Of these, only the last two are in common and extensive use. The chemical scale is indefinite to the extent of the variation in the average atomic mass of oxygen from various natural sources (some 15 parts per million) resulting from variations in the relative proportions of ^{16}O, ^{17}O, and ^{18}O.

Recently, proposals for improving this situation have been made and discussed. These discussions concern (i) how the definition of the chemical scale can best be made more precise, (ii) whether or not the physical and chemical scales ought to be unified, and (iii) whether the definition of the physical scale could perhaps be improved as well.

The most extensive recent discussion of the problem of the mass scales is that of Wichers (*1*), who invites consideration and expressions of opinion by interested persons. Accordingly, we wish to bring into the light of public discussion a proposal which has received considerable consideration in private and which seems to have merit with respect to all three of the afore-mentioned points.

The existence of two sets of mass values differing slightly but significantly, even were the present uncertainty of the chemical scale to be eliminated by a more precise definition, in itself causes some confusion, which is often reflected in pedagogic difficulties. But more serious is the necessity of having two values, one for each scale, of the universal molar "constants," in particular the Avogadro number, the Faraday constant, and the gas-law constant. The necessity of matching the proper value of the Avogadro number with the mass values employed arises especially often in the domain of nuclear chemistry.

Proposals to unify the scales by adopting the physical scale for chemical atomic weights have been regarded with disfavor by many chemists because of the relatively large change, about 275 parts per million, which would have to be made in all of the quantities whose values depend on the size of the mole. There are many physicochemical data whose precision is greater than that and whose values would therefore have to be changed. On the other hand, the serious consideration which has been given by chemists (*1*, *2*) to the proposal of a new unified scale based on $^{19}F = 19$, which would result in a change of 41 parts per million, indicates that many chemists would be willing to accept a unified scale if the atomic weights would not be changed by more than about this amount. There are relatively few chemical data bearing such high precision.

12-Carbon

Fortunately, there is a possible scale definition which, as the basis of a unified scale, would suit chemists and by which, moreover, physicists would benefit greatly. In order to see this we may start by asking why physicists use a relative mass scale and do not give atomic masses in grams. The reasons are twofold: (i) By making a suitable definition of the atomic mass unit, every nuclidic mass can be made very nearly equal to the number A of nucleons in the nucleus, which has obvious advantages. (ii) Masses can be expressed much more accurately in relative than in absolute units (precision of about one part in 10^7 instead of three parts in 10^5). The determination of nuclidic masses to six, seven, or even eight significant figures is more than just an exercise in precision. Slight variations in mass represent relatively large changes in the binding energy of nucleons and may give important information on the nature of nuclear forces. Thus, nuclear physicists will continuously strive to improve the precision because an additional decimal place may reveal hitherto obscure effects of fundamental importance.

Evidently, that definition is to be preferred which allows most nuclidic masses to be expressed with the smallest errors, not only now but also in the foreseeable future. As is shown below, this purpose is fulfilled by taking ^{12}C as the reference nuclide. The best definition of the atomic mass unit is, accordingly,

Mass of ^{12}C equals exactly 12 atomic mass units.

The unit defined in this way is 318 parts per million larger than the present physical mass unit and 43 parts per million larger than the present chemical one.

Physics

To our knowledge, this proposal was first made independently by the physicist A. O. Nier and by the chemist A. Olander (*3*). Although the proposal was not discussed at the 1957 meeting of the Commission on Atomic Weights of the International Union of Pure and Applied Chemistry, a note pointing out the advantages to physicists of ^{12}C as a reference nuclide was appended to the report of the commission (*4*). Briefly, these advantages are the following.

In the mass-spectroscopic determination of nuclidic masses, the most important substandard is ^{12}C. Not only do the doubly, triply, and quadruply charged atomic ions of ^{12}C occur at integral mass numbers so that they can be paired in doublets with nuclides having mass numbers 6, 4, and 3, respectively, but—much more important—no other element besides carbon can be found which forms molecular ions containing as many atoms of but one kind (up to 10 and more). Therefore, the scale $^{12}C = 12$ would allow many more direct doublet comparisons of masses, especially of heavy nuclides, with the reference nuclide than any other scale. 12-Carbon has the ad-

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ditional advantage that carbon forms many more hydrides than any other element, so that an easy reference line for doublets can be produced at almost every mass number up to $A \sim 120$. Many stable nuclides in the mass region $120 < A < 240$ can also be measured in reference to ^{12}C by pairing in doublets their doubly charged ions with singly charged ions of $^{12}C_n$ or of $^{12}C_nH_m$ fragments. Use can then be made of nuclear disintegration data to obtain accurate masses of many other, especially unstable, nuclides.

With ^{16}O as the official reference nuclide, mass spectroscopists have been forced to spend much time and effort in measurements of the ^{12}C to ^{16}O mass ratio, which is needed in order to use their other results. This state of affairs is the more disagreeable since for some reason or other this mass ratio appears to be rather elusive. The proposed definition would put an end to this unfortunate situation.

Chemistry

From a chemical point of view, the opinion has been expressed that the most suitable reference substances for atomic weights are those elements which in nature are anisotopic. However, this seems to be based on an assumption that the only alternatives are natural polyisotopic elements, whose isotopic composition may be variable. If individual nuclides can be accepted, the argument loses its force. The most accurate chemical atomic weights are at present derived from physical data, and for those cases where a chemical determination might still be more precise (for example, Cl, Br, Ag), the physical determination of the atomic weight of a natural carbon isotope mixture in terms of the ^{12}C mass can be made with more than adequate precision; if necessary, the preparation of separated ^{12}C in adequate purity and quantity would be quite feasible.

Conclusion

The adoption of the proposed redefinition of the atomic weight scale should cause very little difficulty for chemists, because the changes would be small in relation to the precision of most chemical data. The present chemical atomic weights can be converted to the new scale by dividing by 1.000043; changes of more than one unit in the last figure of the official 1957 values would be required for only five elements (Cl, Ar, K, Br, and Ag—the latter changing from 107.880 to 107.875). Quantities whose magnitudes are proportional to the size of the mole must similarly be divided by the same factor; in most cases the changes will be insignificant.

For physicists, however, the changes in the mass values would be enormous in relation to the uncertainties in the data, and the new definition would be generally useful for them only if tables of nuclidic masses in the new scale were available. Two of us are considering the compilation of such tables. These data being available, we are inclined to think that neither chemists nor physicists should have objections to the new definition (*5*).

References and Notes

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3. A. O. Nier, University of Minnesota, private communication; A. Olander, Stockholm, private communication.
4. J. Mattauch, Addendum No. 2, Report for 1957, Commission on Atomic Weights, International Union of Pure and Applied Chemistry, *Comptes Rendus de la Dix-Neuvième Conférence, Paris 16–25 Juillet 1957*, p. 153.
5. A more detailed discussion of the problem of the scales of atomic weights and nuclidic masses is being prepared by one of us (J.H.E.M.).

George Malcolm Stratton, Social Psychologist

George M. Stratton, who died 8 Oct. 1957, was the last of a pioneer group of students of the renowned German psychologist Wilhelm Wundt who were credited with having introduced experimental psychology into the United States. True to the experimental emphasis of the Leipzig School, Stratton established and equipped at the University of California one of the first laboratories of experimental psychology in America. Throughout his long and productive career he applied the experimental method to a variety of areas in human behavior, with particular efforts to study the intangible determinants of social behavior.

It was in 1896 that he published his first experimental study. This research was concerned with "vision without inversion of the retinal image" and was the starting point of an extended and significant series of studies in visual space. The original study is today considered a classic in the field. Stratton is credited in this experiment with having "settled both Kepler's problem of erect vision with an inverted image and Lotze's problem of the role of experience in space perception." Although Stratton's studies on binocular vision and depth perception have had the greatest single impact on the thinking and research of his colleagues in psychology, he himself probably considered of greater importance his studies in the fields of emotions, social values, and international conduct. He staunchly believed that the understanding of the impulses, instincts, and motives of man merited the same careful scientific analysis as did sensation and perception, intellect and ability, and learning and memory. It was in these latter fields that most of the early experimental psychologists had directed their energies.

Very early in his career Stratton became interested in studying volitional behavior as it is manifested in drives, impulses, and emotional reactions. Here he found it possible to apply experimental and laboratory techniques to many of the problems, but in studying volitional behavior in social contexts he was confronted with the need of proceeding from the behavior of the individual to the behavior of the group, and group behavior did not readily yield to experimental attack. To these problems of group behavior Stratton brought the objective, factual approach that had characterized his laboratory studies. His theory of group behavior was an extension of the theories he had derived to explain

volitional behavior of the individual, this behavior is modified by interactions among few or many persons in the form of either loosely or tightly knit institutional organizations. His theoretical approach was opposed to the then popular theory of group behavior which postulated "group will" and "group consciousness" as characteristics apart from the individual.

In his book *Experimental Psychology and Its Bearing upon Culture* (1903), Stratton made his first systematic excursion into the realm of social behavior, giving a thorough description of the then known experimental findings in such fields as mental measurements, unconscious ideas, illusions, memory, imitation and suggestion, and color and the fine arts. In connection with each subject he showed the significance the results had for the everyday cultural life of the individual. In this treatment he refuted many erroneous ideas found in the culture of that period, as, for example, that the mental life (mind) cannot be quantitatively described, that experience is a direct impression of external nature upon passive recipients, that the blind person has no space sense, or that the enjoyment of form can be attributed to a mathematical proclivity for certain number relations.

Stratton's first systematic account of volitional responses is found in his book *The Psychology of the Religious Life* (1911). In the absence of any experimental procedure, his approach was a sociophilosophical one. It consisted in a thorough factual accounting of the creeds and motivations to be found in the great religions of the world. In contrast with other early scientists in this field who had restricted their psychological studies to Judaism and Christianity, his analyses included, besides these, Zoroastrism, Taoism, Vishnuism, Buddhism, Confucianism, and the Islamic religions. The book was published at a time when scientists were being severely criticized as godless persons. Stratton believed that an objective scrutiny of religious doctrine was needed and that a comprehensive analysis would yield valuable information on the nature of human impulses, instincts, and motives. Certainly his book gives an accurate and vivid impression of the war of motives that characterizes religious behavior.

Stratton's participation in the psychological program of the Army in World War I increased his interest in the psychology of international affairs and particularly in the problems of world peace. His many publications on these subjects include such titles as "Control of the Fighting Instinct" (1914), "The Psychology of the War Spirit" (1915), "Human Nature and War" (1926), "Re-education for International Affairs" (1929), "Public Opinion in Times of National Crises" (1932), "Problems of War and Peace in the Society of Nations" (1937), "Is World Peace an Attainable Ideal?" (1938), and "Violence between Nations: the Way of Liberation" (1944). In these papers we find applied the same logical and factual approach that characterized his experimental studies. Stratton strongly believed that the time had come when the scientific psychologist should contribute his talents to man's efforts to avert war. A systematic treatment of his points of view in regard to the psychological bases for establishing lasting peace is presented in his book *Man, Creator or Destroyer* (1952). Although written for the general reader, the book presents current scientific knowledge about the creative and destructive aspects of human life and points up the way for man to achieve ultimate peace through a mastery of his creative and constructive powers. From our scientific understanding of the psychological determinants of violent and peaceful actions as seen in children's behavior and in delinquency and crime, industrial disputes, racial segregation, and similar behavioral situations, Stratton develops an inclusive common explanation that he extends to the understanding of the conduct of nations. In simple words, his theory revolves around the establishment of various kinds of communal ties: political, economic, educational, and cultural. In true scientific spirit he produces facts to substantiate his theory in the behavior of two groups of nations, a warless circle of nations and a mutually belligerent group. It is to be noted that in the recent formation, by certain European nations, of a frontier-free common market, and of Euratom as a means of cooperation in developing atomic energy, there is found a demonstration of the kind of communal ties which Stratton years ago stressed as the essential basis for the establishment of lasting peace among nations.

At the time of his death Stratton was completing a book which dealt with "The Divisive and the Unifying Forces of the Community of Nations." Written for the general reader, it gives a comprehensive description of the problems faced today by the society of nations and urges immediate intensification of various unifying forces by which it should be possible to avert further world-wide devastation.

Stratton was held in high esteem by his many colleagues and students. He was very generous with his time, and his practice of psychology frequently extended to helping others in their educational and vocational problems. His lectures on the campus were not only popular but also very telling in their import. Those of us fortunate to have been his associates owe much to his inspirational teaching and leadership.

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News of Science

United Nations Radiation Report

The United Nations Committee on the Effects of Atomic Radiation began a series of meetings at U.N. headquarters on 9 June to review, complete, and approve a comprehensive report to the U.N. General Assembly. The June session of the 15-member committee has before it a working draft of the comprehensive report prepared at the committee's previous session, which ended on 28 February, and further studied by committee members since that date.

The final report, which is expected to be transmitted to U.N. member governments in July, deals with the effects of atomic radiation, including the natural background radiation, artificial radiation from medical applications and other peaceful uses of atomic energy, and fallout resulting from nuclear weapon tests. It is concerned both with the immediate effects on man and his environment and with long-range effects on future generations.

U.S. IGY Meeting

Walter Sullivan of the *New York Times* reported on 1 June that, after turning down a proposal that scientists from the 66 nations taking part in the International Geophysical Year meet in this country, the State Department is understood to have changed its policy. A post-IGY assembly will probably be held next year in or near Washington.

Meanwhile, the Soviet Union has invited IGY participants to meet in Moscow in August to prepare the final rules for exchange of data gained from Soviet and American satellites. While East and West have agreed in principle to exchange such data, they must still agree on how detailed the exchange is to be. Some American scientists have felt that the United States missed a valuable opportunity for building prestige when it rejected the original, informal inquiries on whether such a meeting would be welcome here.

Sullivan's report said:

"The chief factor in the State Department's reluctance seems to have been the prospect of having to accept a delegation from Communist China. The latter has since withdrawn from the IGY in protest at the inclusion of Nationalist China.

"Peiping—and the State Department—were assured by leaders of the IGY that the program was completely nonpolitical. The delegations represent the various national academies of science—not governments.

"State Department sources say, nevertheless, that there is still concern within the department at the prospect that delegations from such unrecognized nations as the Mongolian Peoples Republic and North Korea will wander the streets of Washington.

"Another embarrassment for the State Department is said to result from our diplomats' urging such Chinese Nationalists to apply for visas to Moscow so that the West will be fully represented at the meeting to run there August 1 to 9.

"The Communist bloc has its problems, too. If all IGY participants visit Moscow there will be delegations from North and South Vietnam, East and West Germany, Nationalist China and possibly both North and South Korea.

"The South Koreans recently asked to adhere to the IGY, but since no meeting of the IGY executive body is scheduled before the Moscow meeting, it is doubtful that the application can be accepted before then.

"The enrollment of South Korea would mean that of the four nations partitioned by the cold war, both halves of three would be taking part. The fourth instance—China—underlines an unusual lack of uniformity in Communist bloc policy.

"The Chinese Communists were the first to adhere. When the Nationalists were accepted, to avoid political complications, the two groups were listed in IGY documents as 'Chinese Committee—Peking (Peiping)' and 'Chinese Committee—Taipei.'

"This formula was not acceptable to Peiping, which withdrew on the eve of the IGY. By then Communist China had set in motion an ambitious program which apparently continued with little abatement, despite the withdrawal.

"According to press reports from the Chinese mainland, an oceanographic ship with six laboratories on board was outfitted. The network of fifty weather stations was expanded to 1000. The Academia Sinica—equivalent to our National Academy of Sciences—announced plans to set up a dozen satellite tracking stations from Tihwa, beyond the Gobi Desert, to the sea.

"It also opened a geophysical observatory in Lhasa, Tibet, and a physics research institute in Tihwa. The Tibetan observatory was opened last July 1, the start of the eighteen-month IGY. Networks of magnetic seismic stations were organized and a latitude observatory was established in Tientsin with Soviet help.

"Reports from the IGY World Data Center in Moscow indicate that it is receiving at least some of the fruits of this Chinese effort. The Soviet press has even spoken of Chinese plans to launch earth satellites without saying when this might be done.

"Apart from Communist China all other Soviet bloc nations except Albania are taking part in the IGY. . . ."

New Species of Wasps

Two new genera and approximately 30 new species of wasps whose eggs are laid inside the bodies of other insects, chiefly caterpillars—and which spend all their larval life feeding on the fats and body fluids of the hosts—are described in a recent Smithsonian Institution bulletin. The author is C. F. W. Muesebeck, honorary collaborator of the Smithsonian Institution.

The wasps, technically of the family Braconidae, are found chiefly in subtropical America. They represent one of the most remarkable adaptations of life in nature.

Eggs are deposited by the females inside the host insects by means of an ovipositor, which penetrates the skin, or even the hard scales of beetles. Inside the host the eggs hatch in an environment of relative safety and abundant food. But the larvae do not feed upon, or injure in any way, any of the vital organs of the host. They eat only fats and body fluids that are not essential to life. The host may continue, to all outward appearances, in relatively good health.

But as soon as the larval stage is completed, the wasps break through the body walls, usually killing the host. Metamorphosis of larvae into four-winged wasps usually takes place in two or three weeks.

Scientific Manpower Office

A special Office of Scientific and Technical Manpower has been set up within the Organisation for European Economic Co-operation to direct the organization's drive for increasing the supply of quali-

ed scientists and engineers needed to eet the growing demand of Europe's dustries. Alexander King, while conuing as deputy director of the Euroan Productivity Agency, has been apointed director of the new office and ill report directly to the secretary genral of OEEC. The scientific manpower rogram to be carried out by the office ill be under the direction of a governg committee composed of high-ranking fficials of the 17 European members of EEC, the United States, and Canada.

The organization has already initiated veral projects in this field which will ow come under the direction of the ew office. These include an annual reew of the policies and programs of ember countries for the training of scitists and engineers; a study of techiques for forecasting demands for scitific and technical personnel; and a eries of summer training courses for cience and mathematics teachers. The irst of these courses will be held at Keele, England, and will be open to seected teachers and educational admintrators from any of the member countries. It is expected that it will be followed by two more courses this summer in France and Germany.

Funds for this program and for further action projects to be determined by the governing committee will come from an allocation of $500,000 by the U.S. Government, together with an initial contribution by the OEEC countries and special additional contributions as projects are approved.

Visiting Scientists Program

The National Academy of Sciences–National Research Council has been administering, for the International Cooperation Administration, a visiting scientists program under which more than 200 young scientists at the postdoctoral level have been brought to the United States for 2 years of basic research experience in American universities. Recently ICA asked the Academy–Research Council to continue the program and to extend it to other countries of the world.

The expanded program will bring approximately 150 young scientists to the United States and will serve essentially the same purpose as the original program. However, it will now bring young scientists from countries in Asia, Africa, and South America, as well as from Europe.

The program will be administered by the Office of Scientific Personnel with the cooperation of the Office of International Relations and the Pacific Science Board. Representatives of these three offices of the Academy–Research Council are visiting the countries that are cooperating in the program for the purpose of developing mutually acceptable procedures whereby the scientific academies or equivalent institutions in these countries may assist in the selection of candidates. Walter F. Colby, who has served as assistant to the director of the Office of Scientific Personnel for the Foreign Research Scientists Program, will continue to direct the expanded program for visiting scientists.

Ovulation Test

A new color test for determining a woman's ovulation time, said to have the approval of some Roman Catholic officials, was described in April to the American College of Obstetricians and Gynecologists. Joseph Doyle of Tufts University reported that a special tape, held against the womb, will change color if the uterine secretions contain sugar, which is present only when a fertilizable egg is released from the ovary. Doyle said the procedure, if confirmed, would help make the rhythm method of birth control more reliable.

Soviet Specialists Graduate

The Soviet Ministry of Higher Education reports that Soviet colleges and higher institutes will graduate more than 100,000 specialists in the coming weeks. A complete breakdown of the figure is not available. However the largest single group, 17,000, consists of machine and instrument-making specialists. There will be about 8000 construction engineers, 5000 power engineers, more than 5000 geologists, 5000 chemical technicians, and 3200 metallurgists. The remainder will include "many economists, philologists, biologists, and so forth."

K. G. Nozhko, an official of the Ministry, told the newspaper *Sovetskaya Rossiya* that most of the new specialists would get jobs in Siberia and other eastern sections of the country.

United States Research and Development, 1953–1956

The National Science Foundation reports, on the basis of a recently completed survey, that industrial expenditure for research and development increased more than 75 percent during the three years 1953–1956, and that almost half the costs represented performance for the Federal Government.

"A carefully documented survey, completed for the foundation by the Bureau of Labor Statistics, shows that costs of research and development performed by industry in 1956 were $6.5 billion," said Alan T. Waterman, director, in releasing data from the survey, *Research and Development Costs in American Industry, 1956—A Preliminary Report*. "A similar survey, completed earlier for the foundation by the bureau, showed that industrial research and development cost $3.7 billion in 1953. Many factors have contributed to the increasing growth of industrial research and development during recent years, including the development of new materials and processes completely unknown a decade ago and the increasing use of industrial contracts for carrying out the Federal Government's expanding research and development program. Whereas in 1953 federal contracts for industrial research and development stood at 37 percent of total industrial research and development ($1.4 billion related to $3.7 billion), they rose to almost 50 percent in 1956 ($3.1 billion related to a total of $6.5)."

On the basis of the industrial and Federal Government surveys completed for 1956 and estimates for other sectors of the economy, the National Science Foundation has stated that the country's total research and development effort amounted to approximately $9 billion in 1956, or 67 percent more than the 1953 figure of $5.4 billion. Thus, industry's share in this performance of research and development in 1956, totalling $6.5 billion, was slightly less than three-fourths of all research and development performed in the United States.

A comparison of data from the 1953 survey with data from the more recent survey show the following important changes:

1) Total industrial research and development costs increased 76 percent, from $3.7 billion in 1953 to $6.5 billion in 1956.

2) Research and development financed and performed by industry itself increased 44 percent, from $2.3 billion in 1953 to $3.3 billion in 1956.

3) Federal Government financing of industrial research and development increased 131 percent, from $1.4 billion in 1953 to $3.1 billion in 1956—an increase of from 37 to 49 percent of total industrial research and development funds.

The survey showed that 13 industries accounted for more than 85 percent of all research and development performed by industry in 1956. One-half of all industrial research and development expenditures was concentrated in the aircraft and parts and in the electrical equipment industries. The aircraft and parts industry accounted for 32 percent of the total industrial research and development, and the corresponding figure for the electrical equipment industry was 18 percent. The machinery and chemical industries accounted for 9 and 8 percent, respectively, of the total. The

petroleum products and extraction and the professional and scientific instruments industries each accounted for 4 percent. The remaining industries, separately reported in the foundation's preliminary report, each accounted for less than 3 percent.

Copies of *Reviews of Data on Research and Development, No. 10, May 1958,* may be obtained by writing to the National Science Foundation, Washington 25, D.C.

Scientists in the News

AGNES CHASE, 88, one of the most distinguished of American botanists, received her first college degree on 14 June when she was presented with an honorary degree during commencement ceremonies at the University of Illinois. With no formal training in botany, she became senior botanist of the Smithsonian Institution, Washington. She has spent a life-time studying grasses, and at the Smithsonian she helped build one of the world's most important collections of grasses. She has written extensively.

Retirement in 1939, with the honorary title of research associate, has not stopped her scientific work. She has just finished an index of grass nomenclature and bibliography containing more than 80,000 references that will probably be the standard international authority on this subject for years to come.

KIMBALL ATWOOD, senior biologist at the Oak Ridge National Laboratory, Oak Ridge, Tenn., has been named to head the work in medical genetics at the University of Chicago. The appointment is effective as soon as Atwood can conclude his responsibilities at Oak Ridge.

JOHN T. DAILEY, formerly technical director of research with the Bureau of Naval Personnel, has joined the staff of the American Institute for Research, where he will act as program director for research on the identification, development, and utilization of human talents. He will be working at the Washington office of the institute.

HENRY C. HARRIS, agronomist in charge of the biochemistry laboratory at the Florida Agricultural Experiment Station, has received the Distinguished Service Award of the University of Florida Chapter of Gamma Sigma Delta.

JOHN S. RINEHART, assistant director of the Smithsonian Astrophysical Observatory, Cambridge, Mass., and an astronomy research associate at Harvard University, will become a professor of mining engineering at the Colorado School of Mines next fall. In addition to teaching courses dealing with chemical and physical make-up of explosives, Rinehart will become director of the school's Mining Engineering Research Laboratory.

Sister HILARY ROSS, biochemist Public Health Service Hospital, Carville, La. (National Leprosarium), has received the 1958 Damien–Dutton Award of the Damien–Dutton Society.

President Eisenhower in a congratulatory telegram said: "With her great record in the field of leprosy, she is eminently qualified for the Damien–Dutton Award. Her long service at the Government Hospital in Carville provides an outstanding example of inspired faith and good work."

WILLIAM C. MACTAVISH, who has served New York University for 50 years, has retired as professor emeritus of chemistry. MacTavish, senior member of the faculty, began his career at the university in 1908, when at the age of 15 he was hired as a bottle-washer in the biochemistry laboratory. During his 5 years in the laboratory he learned basic chemistry, and, with only a preliminary public-school education, he compiled enough credits to enter the university as a student. After two leaves of absence for military duty, he received his bachelor's degree in 1924. Two years later, he had earned a master's degree from Columbia University.

MacTavish was a faculty member at both Washington Square College and the College of Medicine through the 'twenties. In 1929 he was made professor at Washington Square College, and in 1930 he became chairman of the department of chemistry. He held this post until 1958.

MacTavish is now visiting toxicologist for Westchester County and special examiner in chemistry for the Civil Service Commission. After a trip abroad, he and his wife will retire to their tree farm in Jackman, Me.

JOHN S. BOYCE, professor of forest pathology at the Yale University School of Forestry since 1929 and chief pathologist of the U.S. Department of Agriculture since the same year, will retire on 30 June. Boyce, whose *Forest Pathology* has been a standard text since 1938, is engaged at present in making a third revision of the volume.

Boyce was born in Belfast, Ireland. Moving to America in early life, he received his bachelor-of-science and master-of-forestry degrees in 1911 and 1912 from the University of Nebraska and took his Ph.D. degree from Stanford University in 1917.

He joined the U.S. Forest Service for 2 years in 1910 and again in 1919, when he became scientific assistant and assistant pathologist in the Division of Forest Pathology of the Department of Agriculture. From 1920 to 1928 he was pathologist in charge of the department's Portland, Ore., branch and for the following year was principal silviculturist and director of the Northeastern Forest Experiment Station.

General DON FLICKINGER has been appointed to the newly established position of special assistant for bioastronautics, deputy commander for ballistic missiles, at the Air Research and Development Command's Air Force Ballistic Missile Division, Inglewood, Calif. The new position involves the planning, control, and use of advanced scientific studies dealing with man and the problems he will encounter in the course of Air Force space studies. Flickinger adds his new duties to his present assignments as ARDC staff surgeon and ARDC director of life sciences.

HARRY E. BACON, professor and head of the department of proctology at Temple University Medical Center, has been presented the highest award given by the Belgian Government. He was made an Officer, Order of Couronne, by King Baudouin, in Brussels on 15 May.

LAURENCE H. KYLE, professor of medicine at the Georgetown University School of Medicine, has been appointed chairman of the department of medicine, effective 1 July. He succeeds HUGH H. HUSSEY, who will become dean of the School of Medicine.

JOHN C. S. PATERSON, associate professor of medicine at Tulane University Medical School, will become chairman of the department of tropical medicine and public health on 1 July.

EMMANUEL FARBER, associate professor of pathology and of biochemistry at Tulane University School of Medicine, has received the second annual Parke-Davis Award in Experimental pathology of the American Society for Experimental Pathology. The award, consisting of $1000 and a medallion, was presented at the annual dinner of the society, held on 16 April in Philadelphia in conjunction with the annual meeting of the Federation of American Societies for Experimental Biology. The award was given to Farber for fundamental research in chemical changes in cells as a cause and basis of disease.

BERNARD L. OSER, president and director of the Food and Drug Research Laboratories, Inc., of Maspeth, N.Y., has received the $1000 Babcock-Hart

ward for his technological services to nutrition and public health. The award was presented to him in Chicago on 27 May during the 18th annual meeting of the Institute of Food Technologists.

WILLIAM GUY has received the Distinguished Service Award of the Virginia Section of the American Chemical Society "in recognition of his outstanding contribution to the professional standing of chemists." A dinner in his honor was held at the Officers Club of Virginia in Richmond on 6 June.

LILLIAN H. MEYER has been named head of the department of chemistry at Western Michigan University, effective 1 September.

WILLIAM BRUECKHEIMER is the new head of the university's department of geography and geology.

CHESTER N. FRAZIER, head of the department of dermatology at Harvard Medical School, will retire from active service at Harvard on 30 June, when he will become Edward Wigglesworth professor emeritus of dermatology. Simultaneously, Frazier will retire as chief of the Dermatological Service at the Massachusetts General Hospital, Boston.

Frazier's contributions to research in dermatology have included studies on the relationship of skin disease to nutrition. He was the first to describe a skin disease caused by Vitamin A deficiency. During 19 years in China (1922–1941), while teaching at the Peiping Union Medical College, he carried on research in skin diseases of the Chinese, and trained Chinese doctors in the treatment of skin disease and syphilis. His studies have also included the biology of syphilis; the effects of race, sex, and hormones in resistance to syphilis; and the mode of action of penicillin.

Frazier attended Wooster College and received the S.B. (1915) and the M.D. (1917) degrees from Indiana University. In 1947 he was awarded the doctor of public health degree by Johns Hopkins University. He also did postgraduate work at the University of Munich, Germany.

AIKOH KAWAHATA, professor of hygiene, Mie Prefectural University School of Medicine, Mie–Ken, Japan, has recently completed a year as Fulbright fellow in the department of physiology and pharmacology of the Kirksville College of Osteopathy and Surgery, Kirksville, Mo. Until February 1959 he will work in the department of physiology and biophysics, University of Washington, Seattle. Kawahata's studies have been in the field of sweat gland physiology.

The U.S. Department of Agriculture honored 182 employees recently in special ceremonies at the sylvan theatre on the Washington Monument grounds in Washington. Eight of these received Distinguished Service Awards, 127 were given Superior Service Awards, and 47 were cited for 40 years or more of service. Those who received Distinguished Service Awards are:

CHARLES DRECHSLER, mycologist, Agricultural Research Service, Beltsville, Md.

GWYNN GARNETT, administrator, Foreign Agricultural Service.

SHERMAN E. JOHNSON, advisor to Agricultural Research Service Administrator.

PAUL V. KEPNER, deputy administrator, Federal Extension Service.

NICHOLAS T. MIROV, plant physiologist, Forest Service, Berkeley, Calif.

ROBERT T. O'CONNOR, physical scientist, Agricultural Research Service, New Orleans, La.

R. LYLE WEBSTER, director of information.

DONALD A. WILLIAMS, administrator, Soil Conservation Service.

Another feature of the ceremonies was the presentation of the William A. Jump Memorial Award to ROBERT S. SHARMAN, a veterinarian of the Agricultural Research Service, for his work in the eradication of foot-and-mouth disease in Mexico following the second outbreak there within the past 11 years.

HARLOW SHAPLEY of Harvard College Observatory has completed his tour as visiting scholar to 50 American colleges and universities. Fourteen of the visits were under the auspices of the American Astronomical Society, underwritten by the National Science Foundation; most of the others were sponsored by the National Organization of Phi Beta Kappa.

RALPH E. HIATT, chief of the Antenna Laboratory at the Air Force Cambridge Research Center, has been appointed research physicist in the University of Michigan Engineering Research Institute, effective 1 July. He will be head of the experimental part of the Radiation Laboratory, which is conducting research in basic electronics, radar and associated devices, antimissiles systems, camouflage techniques, and mathematics.

ARTHUR R. CADE, a member of the L. B. Evans' Son Co. staff, has retired after 18 years of service as chief bacteriologist for the Givaudan Corporation and its subsidiary the Sindar Corporation, of Delawanna, N.J., and New York, manufacturers of organic (synthetic aromatic) chemicals.

F. W. WENT, professor of plant physiology at California Institute of Technology, has been appointed director of the Missouri Botanical Garden, St. Louis, and professor of botany at Washington University, effective 1 September.

Recent Deaths

WILLIAM DENNIS, Middletown Township, N.J.; 85; chairman of the Atlantic Coast Section, Shell Fisheries Division, New Jersey State Department of Conservation and Economic Development, and specialist on the tides and channels of Sandy Hook Bay and the North Jersey coast; 1 June.

Rev. GUSTAVE DUMAS, New York, N.Y.; 59; dean of the Fordham University Graduate School of Arts and Sciences, 1938–51; 28 May.

WILLIAM H. EISENMAN, Cleveland, Ohio; 73; national secretary of the American Society for Metals for 40 years; former head of the chemistry department of Racine (Wis.) College; superintendent of schools in Elmhurst, Ill.; 30 May.

AARON FREILICH, New York, N.Y.; 65; lecturer at Brooklyn College and former chairman of the mathematics department at Lafayette High School, Brooklyn; author of many mathematics textbooks; 1 June.

DANIEL LASZLO, New York, N.Y.; 55; chief of the division of neoplastic diseases at Montefiore Hospital; served at the universities of Freiburg and Cologne, Germany, and the University of Vienna before coming to the United States in 1938; led an investigation of radioactive strontium for the Atomic Energy Commission and the U.S. Public Health Service at Montefiore; 1 June.

KARL PRESSER, New York, N.Y.; 60; radiologist; formerly taught x-ray diagnosis and therapy at the University of Vienna Medical School; 1 June.

GEORGE J. SHIPLE, Detroit, Mich.; 66; professor of chemistry and director of the department of chemistry at the University of Detroit; 18 May.

C. E. SKINNER, Pullman, Wash.; 61; chairman of the department of bacteriology and public health, State College of Washington; 10 May.

IEKOUSSIEL G. TCHERTKOFF, New York, N.Y.; 73; consultant and former visiting physician at the Metropolitan Hospital and at the Sea View Hospital, West New Brighton, N.Y.; specialist in lung diseases; author of many papers on pulmonary tuberculosis; 1 June.

Lord WEBB-JOHNSON, London, England; 77; surgeon to the late Queen Mary of England for 17 years and president of the Royal College of Surgeons for 8 years; 25 May.

Book Reviews

The Tao of Science. An essay on Western knowledge and Eastern wisdom. R. G. H. Siu. Technology Press, Massachusetts Institute of Technology; Wiley, New York; Chapman & Hall, London, 1957. xvi + 180 pp. $4.25.

William James once described a philosopher as a person who can pick a thing up and lay it down again. The book under review suggests that the contemporary scientist is a person who is trying to lay something down while wondering whether he has picked up anything in the first place.

James' definition of the philosopher may be interpreted as meaning that the philosophical method of investigating many different hypotheses for the analysis, description, coordination, and understanding of facts prevents the error of confusing the theoretical description of fact with undescribed fact itself and thereby prevents the philosopher from becoming so obsessed with a particular theory of the facts that he cannot lay that theory down and look at the facts from another theoretical standpoint.

R. G. H. Siu's suggestion (as I interpreted it) that the contemporary scientist is not sure that he picked up anything in the first place is expressed in the three sentences at the beginning of the final chapter of Siu's book (page 145): "Let us distill the previous deliberations. We have touched upon the ancient roots of modern science. We have uncovered the shaky uncertainties on which her facts and methodologies are grounded."

This judgment is reached after a wide reading and analysis of recent studies in the methodology and philosophy of natural science and in the meaning of the value concepts of ethics and the normative social sciences. In his philosophy of the natural sciences, Bridgman's emphasis upon the operational meaning of the concept plays an important role. The theories of Whewell, Mach, Keynes, Feigl, Reichenbach, Wittgenstein, Whitehead, Einstein, and myself are also referred to.

In connection with the theory of scientific concepts of the two last mentioned, Siu's remarks suggest a slight misconception on his part. After describing correctly the theory of scientific knowledge which Einstein and I hold, Siu comments in part as follows (page 18): "Yet it is inconceivable that absolute truth will be given to mere man." Then follows what Siu describes as a "barrage of questions" from which in "contrast to Einstein most scientists have retreated. . . . They have contented themselves with more tangible goals. Prevailing theories are modified in the light of more accurate observations. . . . We cannot extrapolate from thin statistical slices into the remote reaches of space and time" (pages 18–19). It is to be emphasized that there is nothing in the theory of scientific knowledge of Einstein and myself which entails such consequences. The thesis that there are methodological criteria for affirming that the theories of physics are not merely subjective constructs but refer also to the epistemological object of knowledge is in no way incompatible with the fact that the basic assumptions of the theories change with time. A concrete example should suffice to make this clear. Every human being has had the experience of having certain images of a person approaching him at a distance and of interpreting these images initially as a sign of a well-known friend, only to find, as the images become less blurred with the approach of the person, that one has made the wrong hypothesis and that it is not the friend one supposed. From this one does not conclude that there was no objective person present. The same is true of the historical fact of the change of hypotheses concerning the more subtle scientific objects. Maybe, therefore, the scientist has picked up something after all.

In his discussion of value concepts, Siu is equally aware of the major theories and problems. His conclusions will, moreover, stand critical analysis. So also will the criticisms which he draws from these conclusions concerning the adequacy of much of contemporary social science, particularly that part of social science which purports to be able, in the name of scientific method, to make judgments concerning intrinsic or goal values as well as judgments about instrumental values and matters or theories of fact. Social scientists may well ponder the distinction, well known to philosophically minded students of ethics, between instrumental and intrinsic values and its implications for the social sciences. To pay attention to this distinction is to realize that social science must have a method for the determination of intrinsic or goal values that differs from the methods it uses to determine either instrumental values or verified theories of what is the case in fact. A major original contribution of this book is its specification of the consequences with respect to the administration of scientific research and educational institutions, foundations, and laboratories.

In his chapter entitled "Values and incentives in organized research," Siu writes (page 116): "It is unfortunate . . . that overemphasis on consultation and coordination has led to a confounding of values at different levels and of values as a means and as an end. The expertness of an individual in a particular science frequently overflowed into decisions influencing the purposes for which the discipline was but the means." As an example he refers to an army and its military leaders. Their value is instrumental rather than intrinsic or "an end." Their expertness consequently is merely with respect to instrumental values. When, therefore, judgments concerning goal values are made by the military, they are being made by those who lack the expertness to make them. Inevitably, the result, Siu concludes quite correctly, is a "tampering with the free use of scientific disciplines" (page 117) and a "self-injuring confusion in the management of research" (page 117).

But Siu sees that the same errors occur in "industrial concerns and their laboratories and in universities and their research groups"; also (page 116) in the wealthy foundations dispensing grants, when the administrative decision-making heads of these organizations, even though scientifically trained in the scientific methods for making decisions about theories of fact or instrumental values, possess no scientific or philosophical insight into the problem involved in making normative judgments concerning goal values. The danger of corrupting both education and scientific research, in the name of scientific method, because of a restricted conception of method which may be appropriate for solving some problems but is inappropriate for the normative type of problem at hand is indeed very real. The danger is the greater because of the frustrated psychology of many of the directors of research and of the large money-laden foundations—a psychology which Siu describes with realistic concreteness.

What are his positive prescriptions for meeting this situation? Roughly, they are two in number. The first is the supplementation of what he calls the "rationalistic" scientific methods of the West

with the Tao or W[illegible] of No-knowledge of China and Asia g[illegible]rally. Hence the title of his book. Oper[illegible]onally speaking, what this amounts to [illegible] the realization that living experience is an art as well as a science and that it requires the cultivation of intuitive esthetic sensitivity and normatively guided choosing as well as formal theory construction with its indirect operational and experimentally defined methods of confirmation. I welcome this conclusion by Siu because it amounts to an independent confirmation, by a Western trained scientist of Asian name and cultural background, of the analysis of scientific procedures and the interpretation of Taoist and Buddhist Asian culture of *The Logic of the Sciences and the Humanities* and *The Meeting of East and West*.

Siu's second prescription is even more specific and follows from the first. The adequate executive in any social institution, whether it be the government, a university, a research laboratory, or a foundation, must combine an appreciation of the scientific method for solving problems of fact with (i) aesthetic sensitivity to the problematic subtleties of experience and (ii) disciplined philosophical analysis of its problems of value of the two different types. In the words of the title of this book's final chapter, the truly scientific decision-maker must be "The philosopher-executive."

F. S. C. NORTHROP

Yale University Law School

The Admirable Discourses of Bernard Palissy. Translated by Aurèle La Rocque. University of Illinois Press, Urbana, 1957. vi + 264 pp. $5.50.

The only major publication by Bernard Palissy, *Discours admirables*, summarizes the lectures he started to present to a selected group in 1575. The book appeared in 1580 when Palissy was perhaps 70; his birth date is not definitely known. The discourses between Theory and Practice are "on the nature of water and fountains . . . on metals, salts and salines, on rocks, earths, fire and enamels. . . . Plus a treatise on marl, very useful and necessary, for those who practice agriculture . . ."

Aurèle La Rocque presents an introduction and an annotated translation from the rather difficult, antiquated French. This is the book of a practical man who liked to call himself *"ouvrier de terre et inventeur des Rustiques Figulines du Roy et de Monsieur le duc de Montmorency"*—a worker in earth materials and inventor of the naturalistic patterns. He is not a "skeptical chemist" like Robert Boyle in his dialogues (1661), but he criticizes philosophers, alchemists, and certain physicians. From his experiences with the crystallization of salts, the working of clays, and the coloring of glass he derives very definite concepts of good and bad water and their role in the formation of ores, stones, and petrifacts and the operation of salt flats and pumps. He is particularly moving when he describes his long struggles with potteries and enamels, which taught him the difference between "evaporative and accidental humors" and "fixed and radical humors" in clays.

Both the translator and publisher deserve our thanks for this addition to the historical library of the geologist, mineralogist, and chemist.

EDUARD FARBER

4530 Brandywine Street, N.W., Washington, D.C.

Livestock Improvement in Relation to Heredity and Environment. J. E. Nichols. Oliver and Boyd, Edinburgh, ed. 4, 1957. xi + 240 pp. Illus. 16 s.

Few books have been written on livestock breeding in which a successful balance was achieved between genetical theory and actual breeding practice. The volume by Nichols is probably the outstanding example of a happy synthesis in this field. First published in 1944, the book is now in its fourth edition. Its author, who is professor of animal husbandry at the University College of Wales, has had wide experience in Australia and Britain, and this has been of remarkable benefit to his presentation.

After short introductory chapters the author presents the bare principles of heredity and brief discussions of the complexity of genic interaction and of the interplay between genotype and environment. The major substance of the book is indicated by chapters on the following subjects: gene and character frequency; environmental aspects; genetic aspects of (i) selection; (ii) inbreeding as a mating system, (iii) line-breeding; outbreeding and hybrid vigor; mating likes and unlikes; performance and progeny testing; breed construction; and type and environment. There is a concluding review of the present status of animal breeding research and its applications.

The book is written in a clear but condensed style and demands an attentive reader. It should admirably answer its purpose, though one may hope that most students will read it with a broader grounding in genetics than is provided by the author. As complementary reading in courses on general genetics, Nichols' book is to be recommended highly, since it gives an excellent picture of the many and intricate problems associated with the improvement of slowly reproducing animals of economic value. One might wish for somewhat more extensive illustrations. There are a good list of references and a satisfactory index.

WALTER LANDAUER

Department of Animal Genetics, University of Connecticut

The Human Sum. C. H. Rolph, Ed. Macmillan, New York, 1957. vi + 232 pp. Illus. $3.75.

This book deals with two distinct, though related, topics: the changing structure of the family and world population problems. The essays on the family, by C. J. Rolph, Jacquetta Hawkes, Michael Young, Peter Willmott, Edward Blishen, J. M. Mackintosh, James Lansdale Hodson, and Sherwin Bailey, do an admirable job of making clear the changes that have taken place in the structure and emotional flavor of the family unit during the last hundred years, particularly among the lower classes in England. Jacquetta Hawkes epitomizes the present-day urban family as a unit that is "living in its own small box, belonging to no living community and perhaps even ignorant of the names of its neighbors"—a unit that, of all forms of the family so far invented, is "probably the hardest to maintain." This theme is developed further by the other essayists, with little overlap of material. The illustrations by Alfred G. Wurmser enliven an already lively text.

The portion of the book that deals with population problems is somehow less satisfactory. It is not that the essays are not sensible, for they are. Julian Huxley gives a graphic picture of present-day overpopulation; Mary Stocks writes an interesting history of the birth control movement; while A. S. Parkes brings us up to date on recent contraceptive experimentation. Bertrand Russell writes with his usual pithy common sense. It's all good. But it's all been said before. There are so many good books on population problems now—books that remind us of those Dylan Thomas complained about ("books that told me everything about the wasp except *Why*"), books that tell us all that we need to know about population except *what now*? Are we really so unable to see the end of the story? Or are we afraid to describe what we see? Two generations ago there were those who realized that "sex" was a problem, and they sought to grapple with it by writing books just filled with facts about stamens and pistils and pollen and birds and bees. Their facts were true, but these had little relevance to the human problem. Is it not possible that most of what now passes for discussion of "the

population problem" is as peripheral to some real, though unstated, problem as was the birds-and-bees literature to the earlier tabooed subject? Perhaps we should have a moratorium on books on "the population problem" until some rash iconoclast tells us what it is that really bothers us.

GARRETT HARDIN
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University of California, Goleta

Philosophy of Mathematics and Natural Science. Hermann Weyl. Revised and augmented English edition based on a translation by Olaf Helmer. Princeton University Press, 1949. 311 pp. $1.95.

Now available in a paperback printing, the 1949 English edition is, in the main, a translation of an article first published in 1927 as *Philosophie der Mathematik und Naturwissenschaft,* Oldenbourg. It includes many minor changes in the text, additional references, and six essays on developments during the intervening years in mathematics, physics, and biology.

The Story of Albert Einstein. The scientist who searched out the secrets of the universe. Mae Blacker Freeman. Random House, New York, 1958. 178 pp. $2.95.

It was announced some years ago, as the result of an investigation, that if one asked the average man on the street to name several distinguished scientists working in this country, the only name he could recall was that of Einstein. Quite recently typical groups of high-school students were directed, by means of a questionnaire, to give their impressions of the daily life and personal characteristics of the professional scientist.

The results are truly appalling. But to me, at least, it seems quite clear that the statistical average reply constituted a fairly true picture of the work and personal traits of Albert Einstein. Fortunately or unfortunately, Einstein was in no way a typical scientist. He may not have been even a typical genius. He has been called, with some justification, the greatest intellect of the 20th century. But it would be hard to match some of his personal peculiarities.

Where could one find another man of great professional distinction who habitually wore the most informal attire and "forgot" to change to formal clothes when delivering an important invited address at a foreign institution? Who rode by preference in a third-class railway coach and *walked,* carrying his luggage, from the station to the castle of the Queen of Belgium, while the official welcoming committee searched vainly for him in the first-class coaches?

In the book under review, many other classic and apparently true stories concerning Einstein are set forth in simple and dramatic style, occasionally with an added embellishment that the author may have gleaned through her personal acquaintance with Einstein and his friends during several years of his sojourn at Princeton. Mae Blacker Freeman, wife of a well-known physicist, wisely makes no attempt to describe or discuss the scientific discoveries that brought such world fame to Einstein. Her book, which is designed for young readers, is confined almost solely to Einstein as an unusual human being. Relatively full treatment is given, appropriately, to his early life, prior to his first university position.

It has always seemed to me that the most significant part of the life history of any acknowledged genius is just this early period. What were the circumstances that encouraged, or at least allowed, the unfolding of great intrinsic talent? So often it seems as though only a fortuitous combination of circumstances has served to avert the suppression of such development. Einstein, quite possibly because he was a Jew, was unable to get a university position in Switzerland after his graduation from a Swiss institution. But he did finally obtain a routine position in the Swiss Patent Office, where the daily work assigned to him was dispatched in a comparatively short time, leaving the major part of the day free for him to "think." Here the theory of relativity was developed—a product of pure thought, although based of course on sound knowledge of experimental facts.

The latter portion of Einstein's life was occupied, in great part, with his endeavors to help those of his own race, all over the world, and to advance world peace. He was an admitted pacifist and conscientious objector, with a complete contempt for the military forces ("the vilest offspring of the herd mind"). Yet it was Einstein's personal appeal to President Roosevelt that set in motion the huge research project that led to the atomic bomb as well as to industrial uses of nuclear energy.

The hero worship that enveloped Einstein in his later years is almost unbelievable. He is, so far as I know, the only scientist who has ever rated a ticker-tape parade along Broadway or who has been greeted with a storm of applause when he appeared in a box at the Metropolitan Opera. These and many other details of such hero worship during his tours all over the world are duly set forth, with a minimum of emphasis on the slanderous attacks also made upon him.

Presumably this little book is designed to inspire young students to choose a scientific career. But when the results of the questionnaire already mentioned are taken into consideration, the effect may well be just the opposite. The personal life of Einstein surely makes interesting reading, but it is far from the life of the typical scientist and far from one that even the typical scientist would care to emulate. Most scientists, so far as their personal lives are concerned, are quite normal human beings, a fact concerning which the average high-school student now seems completely uninformed.

RAYMOND T. BIRGE
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University of California

Biological Aspects of Cancer. Julian Huxley. Harcourt, Brace, New York, 1958. 156 pp. $3.75.

The evolution of this book is as interesting as the subject matter. It began as a lecture, grew into two reviews, and emerged in its present form. The stimulus for this development was the discovery of "so many unfamiliar facts" which led to "so many ideas novel to . . . a biologist" that the author came to the realization that "cancer, far from being a field of purely medical concern, is a key subject for general biology in its present stage of development." While this conclusion is applicable to other problems of medical science, it is heartening to know that a biologist of Huxley's stature emerges from his "hard labour" with an opinion shared by oncologists interested in the biological aspects of cancer.

The volume should be read by biologists who are unfamiliar with the cancer process and, of more importance, by all who are engaged in cancer research, regardless of their previous training. The summary should be read first to acquaint the reader with the broad objectives and conclusions of the author. Huxley's interests include the fundamental factors involved in the origin and growth of cancer, the various agents responsible for the occurrence of the disease in experimental animals and man, present-day theories of the origin of the cancer process, and practical procedures for treatment and prevention. No book of this size could include all biological aspects of cancer, but this is a well-balanced discussion.

Oncologists will find statements in support of the opening sentence that the author "has never done original

work on the subject" and several misspelled names, but these are of little importance when the author's thesis is to bring about the "amalgamation of cancer research with biology."

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Reports

Experimental Mucormycosis in the Healthy Rat

In a study of inflammation we observed germination and growth of *Rhizopus oryzae* spores which we had injected into Selye's "granuloma pouch" (*1*) in the healthy rat. Wright *et al.* (*2*) used this technique to study growth of another fungus, *Coccidioides immitis.*

Twenty-five-milliliter pneumoderms were produced on the backs of Wistar strain rats that weighed from 150 to 200 g each (*3*). A 0.5-milliliter saline suspension of 1,250,000 spores (estimated by hemocytometer count) of *Rhizopus oryzae* (Duke University, No. 2473) was injected into each pouch. Rats were sacrificed after 24 hours, 3 days, 6 days, 10 days, 4 weeks, and 6 weeks, respectively, and tissues were taken for histologic sections and cultures on Sabouraud's medium from the pouch, lungs, spleen, liver, and pancreas.

In all rats killed 24 hours after injection of spores, there was a gray-yellow exudate within the hyperemic, edematous, thickened wall of the pouch. In those killed after 6 days, yellow, cloudy fluid had accumulated and small gray-yellow nodules, 3 to 4 mm in diameter, appeared in a thickened wall. In those killed at the end of 4 weeks, several of the nodules appeared to have merged, with formation of raised gray-yellow areas in the wall of the pouch. The fluid in the pouch had not become grossly bloody. In one of the animals sacrificed at the end of 4 weeks, the pouch had collapsed and the small nodules had become walled off by fibrous tissue. No fluid was present. In the animals killed after 6 weeks, the pouches were collapsed, but the sites were marked by small, firm gray-white subcutaneous nodules.

Rhizopus oryzae grew in cultures from the pouch, but not from any organ, of each rat when sacrificed. Species of *Penicillium, Aspergillus,* or *Trichoderma* grew in cultures made from the lungs of most animals, but all cultures from other visceral organs remained sterile. It is felt that fungi in the lung were air-borne contaminants in the alveoli, as no fungi were demonstrated to be present in the histological sections.

In histological sections of the pouch at the end of 24 hours, there were hyperemia and edema. A granulocytic exudate was present, and hyphae of the fungus were present in the wall. After 6 days a granulomatous reaction was evident in the wall of the pouch. Nodules containing centrally located hyphae of the fungus and degenerating neutrophils were present. This central area was surrounded by multinucleated cells and macrophages or epithelioid cells. There was also a moderate degree of fibroblastic proliferation. At the end of 4 weeks fibrous tissue proliferation was more pronounced and surrounded or walled off the nodules; hyphae of the fungus were still present. There was very little difference between the 4- and 6-week-old pouches except for perhaps a slight increase in fibrous tissue.

Histological sections of the viscera were not remarkable except for the lungs, which in some of the animals contained very early lesions of so-called "enzootic bronchiectasis" (*4*). It has been stated that approximately 75 percent of rats, particularly of certain strains, have this lesion in their lungs by the time they are a year old (*5*). No fungi were seen in any of the organs sectioned.

Infection with *Rhizopus oryzae* was induced in a pneumoderm or Selye's pouch on the backs of normal healthy Wistar strain rats. The fungus excited a nonspecific inflammatory reaction which after 6 days became granulomatous in type, with the formation of gross nodules. Infection had continued for at least 6 weeks in the pouch but had not spread to the viscera; all histological sections and cultures of the latter were negative for *Rhizopus oryzae.* In several of the 4- and 6-week-old rats the inflammatory process had apparently subsided, at least to some degree, and the nodules had become walled off by fibrous tissue. This may indicate that the animals were beginning to overcome the infection. However, experiments are now in progress to see how long the animals will remain infected with the fungus.

The Selye's pouch offers an excellent tool for the study of mucormycosis in a physically defined space. The use of normal rather than metabolically altered animals may facilitate the understanding of the pathogenesis of this mycotic infection in human beings (*6*).

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29 January 1958

Evidence for a Blue-Sensitive Component in the Retina of the Gecko, Oedura monilis

In October 1955 there arrived from Australia nine living specimens of the gecko, *Oedura monilis.* A digitonin extract was prepared in accordance with the usual methods of visual pigment research (*1*). The alkaline extract was divided into two 0.5 ml portions. One of these was analyzed directly while to the second portion was added 0.05 ml of 0.1*M* NH_2OH. Both portions were then analyzed by the method of selective bleaching (*1*). The results of both analyses led to the same conclusions, so only the data obtained in the NH_2OH experiment will be summarized. The spectrum of the unbleached extract (Fig. 1, top, curve 1) is typical of a solution of visual pigment. After exposure for 124 minutes to a light at 606 mμ, the pigment was bleached to give curve 2 (Fig. 1, top). The selective change in density is given by the difference spectrum (Fig. 1, bottom, curve 1). This NH_2OH difference spectrum suggests that the retinal extract contained a photolabile component with an absorption maximum at about 518 mμ. Retinene is most probably the chromophore of this pigment. This is indicated by the fact that the oxime of retinene$_1$ (formed by adding NH_2OH to a solution of crystalline all *trans*-retinene$_1$ in digitonin) yielded a spectrum (indicated by *X*) which, when scaled properly coincided with the product of bleaching.

All technical papers are published in this section. Manuscripts should be typed double-spaced and be submitted in duplicate. In length, they should be limited to the equivalent of 1200 words; this includes the space occupied by illustrative or tabular material, references and notes, and the author(s)' name(s) and affiliation(s). Illustrative material should be limited to one table *or* one figure. All explanatory notes, including acknowledgments and authorization for publication, and literature references are to be numbered consecutively, keyed into the text proper, and placed at the end of the article under the heading "References and Notes." For fuller details see "Suggestions to Contributors" in *Science* 125, 16 (4 Jan. 1957).

The results suggest that this gecko contains in its retina a photosensitive pigment comparable to that found in other geckos (*1*). This unusual group of visual pigments is characterized by absorption spectra located, not in the general region of 500 mμ, the typical position for the visual pigments of terrestrial animals, but in the general region of 520 mμ. The pigment from *Oedura* is very probably a visual pigment. This is indicated by its photosensitivity, by the presence of retinene, and by the fact that the difference spectrum agrees well with the construction based on Dartnall's nomogram (*2*) for the visual pigments. The possible biological significance of this group of visual pigments has been discussed in a previous report (*1*).

The spectrum obtained (Fig. 1, top, curve 2) after the initial bleaching with light at 606 mμ is very informative because it possesses a distinct upward inflection in the region of 460 mμ. This is an important point because it suggests the presence, in the solution from which the 518_1 pigment had been removed, of a blue-absorbing component. The next two exposures show that this inflection was abolished by light of wavelength shorter than 606 mμ. The first of these exposures was to light at 560 mμ. This caused a very small change, but the important information conveyed by this bleaching was that the 518_1 pigment had, in fact, all been removed by the previous long exposure to light at 606 mμ. The final exposure was to white light. This led to a disappearance of the inflection (Fig. 1, top, curve 4). The difference spectrum which resulted from these two final bleachings (Fig. 1, bottom, curve 3) revealed the occurrence of a small but definite density loss, maximal at about 457 mμ, and a density gain at about 360 mμ.

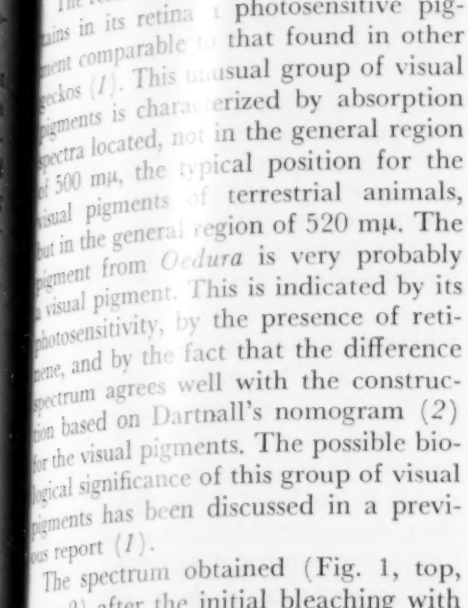

Fig. 1. (Top) Curve 1, absorption curve of unbleached extract: curve 2, result of exposure to light at 606 mμ; curve 3, result of exposure for 125 minutes to light at 560 mμ; curve 4, result of exposure to tungsten light (40 watts) for 10 minutes. (Bottom) Corresponding difference spectra. Curve 1 is the 1–2 difference spectrum. The points indicated as *X* show the data obtained with a retinene$_1$ oxime in 2-percent digitonin. Curve 2 is the 2–3 difference spectrum; curve 3, the 2–4 difference spectrum.

The evidence supports the idea that a blue-sensitive pigment was in fact a component of the original retinal extract of this gecko. The results of both experiments point to the fact that, following removal of the 518_1 pigment, there occurred in response to appropriate illumination a selective change as described. The important feature of this finding is that it was obtainable in an extract containing NH_2OH, a substance which is known to be useful in preventing isomerizing or other side reactions of the products of bleaching. Moreover, the presence of a clear inflection in the spectrum following the removal of the 518_1 pigment is evidence which cannot be easily ignored. The only point in question in this case is whether the blue-absorbing pigment was a component of the original unbleached extract or was a product formed as a result of bleaching the 518_1 pigment. It could, for example, have been a regenerated pigment. Hydroxylamine, by combining with retinene, effectively prevents regeneration in extracts of the retina. In any case no evidence was obtained of regeneration during the 45-minute period which was required to determine the absorption curve. The argument that a secondarily formed photosensitive pigment was present is not easily disposed of; to refute it would require experiments in which the main pigment is left untouched. Whether such experiments can ever be satisfactorily carried out is questionable; lack of animals has thus far prevented attempts in this direction.

A comparison of the difference spectra of this presumed blue-sensitive pigment (a pigment with peak absorption at 457 mμ was assumed) with the curve constructed from Dartnall's nomogram (*2*) shows that the data fit well, considering the small magnitude of the difference spectra. This agreement accords with the idea that the *Oedura* retina contains a visual pigment with absorption in the blue region of the spectrum. The photolabile substance riboflavin has been detected in the vertebrate retina (*3*). It is clear, however, that the blue-sensitive pigment of *Oedura* is not riboflavin. A solution of riboflavin in 2-percent digitonin yielded, after illumination, a difference spectrum with two peaks, one at 373 mμ and the second at about 453 mμ. The 453 mμ was significantly narrower than the 457 mμ spectrum of *Oedura*.

The literature on visual pigments includes a number of references (*4*) which suggest the occurrence of blue-sensitive pigments in the retinae of various vertebrates. Some of these claims are based on inadequate or even questionable experimental procedures, so it is not surprising that the claims have provoked criticism. This report, which is the first account of a blue-sensitive component in the retinae of lizards, is unique for two reasons: (i) Isomerizing actions, which could confuse the interpretation, were reduced to a minimum, and (ii) the pigment in question was demonstrated to be present in the extract before the bleaching employed to remove it.

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5. This study was aided by a grant from the Division of Research Grants and Fellowships, National Institutes of Health, U.S. Public Health Service, and by a grant from the University of California Board of Research.

10 December 1957

A Mechanism for Pyridine-Nucleotide-Dependent Dehydrogenases

Most dehydrogenases, which utilize DPN (*1*, *2*) or TPN as hydrogen acceptors, can be classified as either alcohol or aldehyde dehydrogenases. A large body of evidence, accumulated in recent years, indicate that fundamental differences exist between these two classes of enzymes. The enzymes which can be classified as "alcohol" dehydrogenases oxidize primary alcohols to aldehydes, secondary alcohols to ketones, primary amines to ketones and ammonia, and hemiacetals to lactones (*3*). Examples of each subgroup are alcohol dehydrogenase, lactic dehydrogenase, glutamic dehydrogenase, and glucose-6-phosphate dehydrogenase, respectively. The mechanism proposed in this paper is meant to be applicable only to this group of enzymes and is not to be applied to aldehyde dehydrogenases.

In recent years great effort has been expended in elucidating detailed properties of these dehydrogenases. Among the many significant findings, some require special enumeration. First, the enzymes act by direct hydrogen transfer in a stereospecific fashion both with respect to the substrate and with respect to the pyridine ring (*4*). The reversible oxidation-reduction site in the coenzyme molecule is the *para* position of the pyridine ring (*5*). Second, it has been shown that

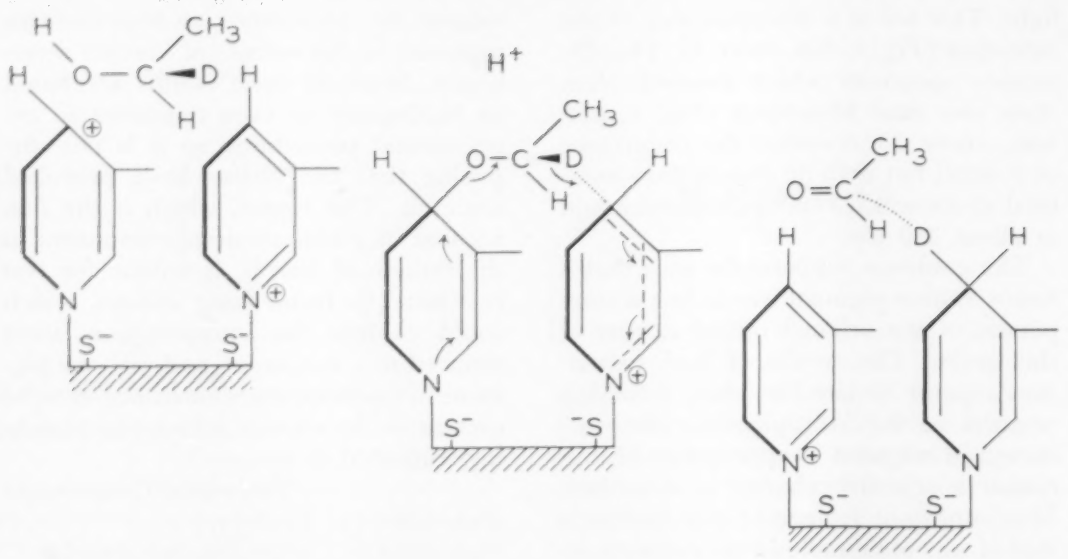

Fig. 1. Proposed mechanism of hydrogen transfer from substrate to coenzyme. The substrate is depicted, for clarity, as deutero-ethanol.

in many instances the presence of a dehydrogenase will promote a much more favorable addition reaction between nucleophilic substances and the pyridine coenzymes, as compared with the extent of addition in the absence of enzyme (*6*, *7*). The nucleophilic substances which show this favored addition fall into two classes (*7*): (i) ions, such as sulfide, bisulfite, and cyanide, are bound with a stoichiometry of 1 mole per mole of DPN bound to the enzyme, and (ii) substances which resemble the substrate in structure are bound with a stoichiometry of 1 mole per 2 moles of DPN bound maximally. All substances which show this favored addition on a dehydrogenase are competitive inhibitors of the corresponding substrates. Finally, a number of these dehydrogenases are known to be zinc proteins (*8*), though some do not appear to contain this metal, notably rat liver lactic dehydrogenase (9).

To explain the above observations, the following unified theory for the mechanism of action of the "alcohol" group of dehydrogenases is offered for consideration. This theory is schematically summarized in Fig. 1. The enzyme binds 2 moles of DPN or a multiple thereof. The substrate, depicted here as monodeuteroethonal, binds through the hydroxyl group (or amino group) to the *para* position of one of the DPN molecules, with the release of a hydrogen ion. The second molecule of DPN will then accept a hydride ion (in this case a deuteride ion) from the substrate, thereby being converted to the reduced form. This transfer of a hydride ion can be realized by a simple electron shift from the nitrogen of one DPN molecule to the nitrogen of the second molecule. When the product is released, the first DPN molecule is regenerated. Both DPN molecules are equally effective in accepting substrate or hydrogen. It must be emphasized that no bond between substrate and protein is required.

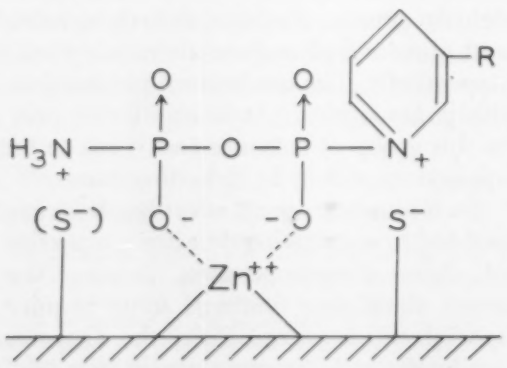

Fig. 2. Suggested generalized mode of binding of DPN to the dehydrogenases. The adenine amino group may or may not be bound to a sulfhydryl grouping. The zinc binds the pyrophosphate, and the pyridinium nitrogen is linked to a sulfhydryl group.

This mechanism is consistent with the experimental observation of direct hydrogen transfer between substrate and coenzyme. Since addition complexes between nucleophilic agents and DPN form at the same position of the DPN molecule as does the substrate complex, the ternary complexes between enzyme, coenzyme, and substrate analog are understandable. This would also explain the competitive inhibition observed between the substrate and the complexing agents. The mechanism is in accord with the discrepancy observed in stoichiometry between ions such as cyanide and bisulfite and agents which resemble the substrate in structure. Lastly, the theory takes into account the known site of reduction of DPN and also the position of the pyridine ring where formation of the complex occurs.

Kinetically, this picture results in a complex series of equilibria, which can only be approximately treated by simplified forms of rate equations. The abnormal kinetic behavior of the dehydrogenases, especially their substrate inhibition, would be a logical consequence of this mechanistic picture.

As indicated above, this mechanism would imply the following sequence of reactions:

$$E + \text{DPN} \rightarrow E\text{-DPN} \quad (1)$$

$$E\text{-DPN} + \text{DPN} \rightarrow E\text{-DPN}_2 \quad (2)$$

$$E\text{-DPN}_2 + S \rightarrow E\text{-DPN-DPN}.S + \text{H}^+ \quad (3)$$

$$E\text{-DPN-DPN}.S \rightarrow E\text{-DPN-DPNH} + P \quad (4)$$

$$E\text{-DPN-DPNH} + \text{DPN} \rightarrow E\text{-DPN}_2 + \text{DPNH} \quad (5)$$

$$E\text{-DPN-DPNH} + S \rightarrow E\text{-DPN}.S\text{-DPNH} + \text{H}^+ \quad (6)$$

$$E\text{-DPN-DPN}.S + S \rightarrow E\text{-DPN}.S_2 + \text{H}^+ \quad (7)$$

where E stands for enzyme, S for substrate, and P for product. There will be a competition between reactions (*5*) and (*6*) and reactions (*4*) and (7). The product from reactions (*6*) and (7) will result in an inactive species of enzyme; thus, the results of reactions (*6*) and (7) appear as substrate inhibition. Reaction (*3*) depends largely on the affinity of the coenzyme for the substrate. For this reason the affinity will differ with different coenzymes. From reactions (*2*) and (*3*) it is clear that the affinity for the substrate depends on the DPN concentration.

Zinc is not an integral part in the actual catalysis, but its presence can be easily explained on a functional basis by assigning to the radical the role of binding site of the pyrophosphate moiety of the coenzyme (Fig. 2).

In this mechanism the role of the protein in the over-all reaction is to impart specificity to the dehydrogenase action by determining the relative distance and spatial arrangement of the DPN molecules. A specific geometrical configuration of the DPN pair will also result in

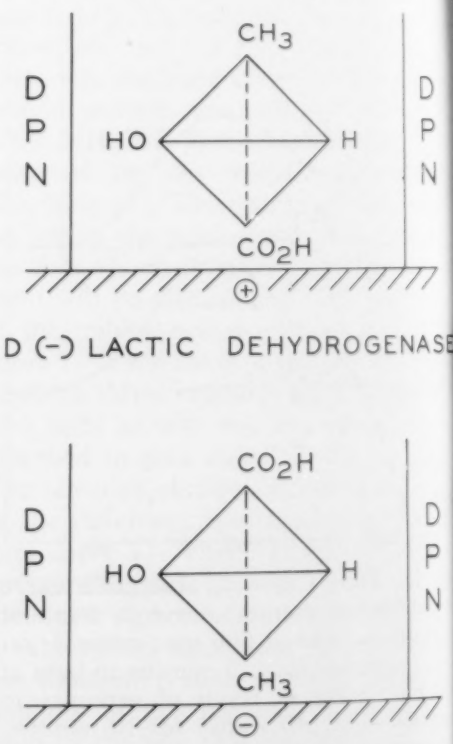

Fig. 3. Possible role of the protein in determining enzyme specificity. The relative arrangement of the DPN molecules determines substrate specificity. The protein itself partially determines stereospecificity.

...reospecificity, both with regard to substrate and with regard to coenzyme. A further role of the protein can be envisioned and is illustrated in Fig. 3. Given identical spatial arrangement of the DPN molecules, the presence of a negatively charged grouping on the enzyme would convert a D-lactate specific enzyme into an L-specific enzyme.

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1. Abbreviations: DPN and DPNH, oxidized and reduced diphosphopyridine dinucleotide, respectively; TPN, triphosphopyridine dinucleotide.
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9 January 1958

Production of Sterility in Mice by Deuterium Oxide

The present availability of D_2O at a reasonable price has stimulated an increased investigation of its physiological effects. The inhibition of ascites tumor growth and algal reproduction has recently been reported by this laboratory (*1, 2*) and others (*3*).

We have demonstrated the production of sterility in mice by the substitution of D_2O for a part of the drinking water (*4*). In the first experiment, six female and six male C_{57} mice that had been maintained on 30 percent (*5*) D_2O in the drinking water for 4 weeks were mated (*6*). The animals were housed, three females and three males to a cage. Administration of D_2O was continued for 10 weeks. Since there were no pregnancies at the end of this time, the D_2O was discontinued. At the end of another 8 weeks, there still being no pregnancies, three of the treated females were mated with three normal males, and three of the treated males were mated with three normal females. Although the mating of D_2O-treated females with normal males resulted in litters at the end of 3 weeks, all offspring died within 24 hours, and two of the mothers died. The mating of D_2O-treated males with normal females did not produce offspring until the end of 13 weeks, when one female littered. From the three D_2O-treated males and three D_2O-treated females remaining together, one female littered in 4 weeks, one in 10 weeks. This experiment is graphically represented in Fig. 1.

In the second experiment, both C_{57} and Swiss mice were used. A minimum of ten mice of each sex of each strain were maintained on 5, 20, or 30 percent D_2O in the drinking water for 8 weeks, as is indicated in Table 1. At the end of this treatment period, administration of D_2O was discontinued and each mouse was individually mated with a normal mouse of the same strain. At the same time, normal mice of each strain were individually mated as controls. The animals were not handled or disturbed during the first week after parturition, since handling of the young can lead to cannibalism by the parents. Offspring were counted and sexed 2 weeks after birth.

Those pairs which did not produce a litter during a 28-day period after the beginning of mating are considered to be a sterile pair (Table 1). This period allowed 1 week more than the average 21-day gestation period.

Our data, summarized in Table 1, indicate that 30 percent D_2O causes 100 percent sterility (*7*) in both C_{57} and Swiss males. Some of the C_{57} females were also sterile. These data and the unpublished results of another series suggest that 20 percent D_2O in the drinking water for C_{57} males produces almost complete sterility and that 5 percent D_2O in the drinking water of C_{57} mice appears to produce a degree of sterility comparable to that which 20 percent D_2O achieves in Swiss mice.

Those pairs in which the males had received 20 or 30 percent D_2O and which had no litters during the first 28 days began having litters after 45 days of mating, indicating that the sterility produced by D_2O is slowly reversible (*8*).

We found no significant difference in the litter size or the sex ratio from that of the controls. This is in contrast to the effect of radiation which shows, in addition to the sterility effects (*9*), reduction in the litter size and a change in sex ratio (*10*). The failure of Hansen and Wülfert (*11*) to observe sterility in mice as a result of administration of D_2O is probably due to the low concentration employed by them.

Table 1. Effect of D_2O on the fertility of C_{57} and Swiss mice. D_2O was added to the drinking water.

Sex	D_2O concn. (%)	Pairs (No.)	Pairs Sterile (%)	Offspring Av. No.*	Offspring Av. No. per mating
		C_{57} mice			
Controls	0	24	17	6.4	4.4
Male	5	10	30	5.4	3.8
Female	5	10	30	7.6	3.8
Male	30	19	100	0	0
Female	30	10	40	5.2	2.6
		Swiss Mice			
Controls	0	19	5	9.0	8.5
Male	20	10	40	9.3	5.6
Female	20	11	0	9.1	9.1
Male	30	10	100	0	0
Female	30	10	0	9.1	7.3

* Calculated as the total number of offspring surviving 2 weeks divided by the number of litters containing live offspring at 2 weeks.

Several physiological mechanisms by which D_2O produces sterility in mice can be suggested. D_2O could interfere with maturation of the ova or sperm, or possibly reduce sperm motility. There is also an indication from our results that D_2O may interfere with the proper development of the fertilized ovum. It has been shown that D_2O inhibits the cell division of algae (*2*). This observation suggests that the most likely points of susceptibility would be the development of the sperm and the division of the fertilized ovum. That the former may be the more probable is supported by the greater susceptibility of the males to D_2O and, theoretically, by the known difference between males and females in the generation of germ cells.

On the biochemical level, hydrogen bonding is important for the maintenance of the secondary and tertiary structure of many biologically important macromolecules. Even a small difference in the properties of a proton bond and a deuteron bond might be expected to induce

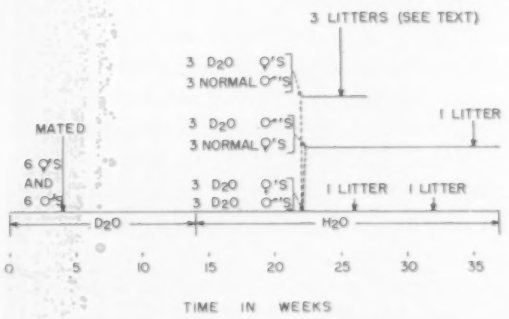

Fig. 1. Production of sterility in C_{57} mice by administration of 30 percent D_2O in the drinking water. Horizontal arrows indicate the length of time animals were given D_2O or H_2O. Vertical arrows indicate the time of mating or littering. Dotted lines indicate the time of cross-mating of treated and normal animals, as described in the text.

large effects in macromolecules and particularly in their function. Aberrations in the structure of deoxynucleic acids may be of especial importance because of the role of these acids in gene structure and cell division.

Further work is in progress to find other physiological effects of D_2O and to understand the mechanism of these effects in biochemical terms.

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5. All D_2O dilutions were made up as volume-percent, approximately equal to atoms-percent of the total hydrogen present.
6. As used in this paper, the term *mated* indicates that males and females were housed together continuously.
7. *Sterility*, as used in this paper, means the inability to produce visible pregnancy.
8. *Note added in proof*. These mice sired five litters which were born 45 to 49 days after D_2O administration was discontinued, 18 litters which were born during the 57- to 76-day period following withdrawal of D_2O, and only three litters which were born in the following 5-week period. Females of the five remaining pairs were not pregnant at 105 days [W. L. Russell, "Genetic effects of radiation in mammals," in *Radiation Biology*, A. Hollaender, Ed. (McGraw-Hill, New York, 1954), vol. 1, pt. 2, pp. 825–859].
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9 December 1957

Dating of Zawi Chemi, an Early Village Site at Shanidar, Northern Iraq

Briefly mentioned by Robert Braidwood in his article, "Near Eastern Prehistory" (*1*), the open village site of Zawi Chemi Shanidar, situated in northern Iraq, has recently provided material for a carbon-14 date. The charcoal sample was dated as 10,870 ± 300 years before the present at the radiocarbon laboratory of the U.S. Geological Survey and bears the laboratory number W-681. This site, tested under Smithsonian Institution sponsorship (*2*), has two occupations, totaling a depth of 1.5 m. The upper, layer A, is of post-Christian date. The lower, layer B, in which the sample was recovered, contained a preceramic industry which may be generally equated with "early Neolithic" or Braidwood's "incipient cultivation." The sample (*3*) was picked from a broad charcoal streak at a depth of 1.2 m, well within layer B. Some tree rootlets were observed in various parts of the excavation, but no contaminating rootlets were seen in the immediate area of the sample.

A few centimeters beneath the locus of the sample and to one side was found a roughly circular enclosure about 3 m in diameter, composed of river cobbles and field stones. It looks like an example of primitive architecture. The carbon date would appear to give an approximate age for this feature.

The same material culture was found in the upper stratigraphy of Shanidar Cave, about 4 k away, which provides a reasonable basis for assuming contemporary or seasonal occupations at both sites. Furthermore, an age of 10,600 ± 300 years before the present (W-667) was determined from a charcoal sample from the top of layer B of the cave, or Shanidar B1. This is just beneath and somewhat intermixed with material from the base of layer A, where close resemblances with Zawi Chemi B are found.

Charcoal is known to absorb carbon from humic acids in circulating ground water. This contaminating carbon can be older or younger than the charcoal, depending on its source, but a younger source is more likely in most situations. For this reason, samples being prepared for radiocarbon dating are boiled in solutions of HCl, then in NaOH, and finally in HCl again. The material extracted in the alkali treatment consists of the humic acid and lignin fraction which can contain the transported "foreign" carbon. This portion is not included in the C^{14} analysis. Generally, only the remaining material is used. However, the sample from the open village site (W-681) was found after separation to be too small for an analysis, and so both fractions were combined for the run. The error quoted after the age does not include the possibility of foreign contaminants, which is impossible to assess, but as is customary, merely gives the counting error due to random disintegrations. The sample from the cave (W-667) yielded sufficient material for a normal analysis.

Karim Shahir, an open site excavated by the University of Chicago Oriental Institute team and situated about 160 k to the southeast, has an industry which is rather like that from Shanidar Cave and the Zawi Chemi village site. We can say tentatively, on the basis of the present evidence, that Karim Shahir, the related sites, and the Shanidar occupations are culturally as well as chronologically related (*4*).

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21 May 1958

Nucleolar Chromosome in the Rust Fungus Scopella gentilis

Allen (*1*), while working with *Puccinia malvacearum* Bert., first surmised that in rust fungi the nucleolus is probably organized on some definite locus of a chromosome. Olive (*2*), in a cytological study of *Coleosporium vernoniae* Berk. & Curt., noted that during the early stages of meiosis a nucleolus organizing chromosome is discernible. The morphological details of the chromosome as found in rust fungi, however, do not seem to have been fully elucidated. Singleton (*3*), in *Neurospora crassa* Shear & Dodge, has demonstrated the occurrence of a satellite chromosome associated with the nucleolus during mitosis as well as meiosis. The satellite zone [SAT-zone (*4*)] during pachytene, however, was not clearly demarcated, and it was observed that either due to the close proximity of the "b chromomere" to the satellite or due to its being distantly located, the latter frequently lost its identity. In the chromosome map of *N. crassa* this was designated "chromosome 2" because it was the second longest in a complement of seven.

In the study reported here (*5*) it was determined that *Scopella gentilis* (Syd.) Mundk. & Thirum. possesses a haploid complement of eight chromosomes. It was found that in early diplotene the nucleolar chromosome pair, because of its distinctive morphology, could be easily differentiated from the other seven pairs (Figs. 1–3). Inasmuch as the present observations are based on aceto-orcein preparations, the nucleolus is barely visible as an unstained globular refractive body (Fig. 3, arrow). The satellite-zone consists of a pair of stalked satellites or trabants followed by a swollen knoblike region. Its resemblance to the heterochromatic region found in the nucleolar chromosomes of certain higher plants is quite suggestive. The short arm, apart from bearing the nucleolus organizer, also possesses two interstitial chiasmata (Fig. 2). A small achromatic gap can be observed just where the long arm of the chromosome begins (Fig. 2, arrow) which possibly denotes the centromere position. The chromosome in diplotene measures about 8.5 μ, and it is the longest pair in the whole complement. In the upper focus of the microscope the

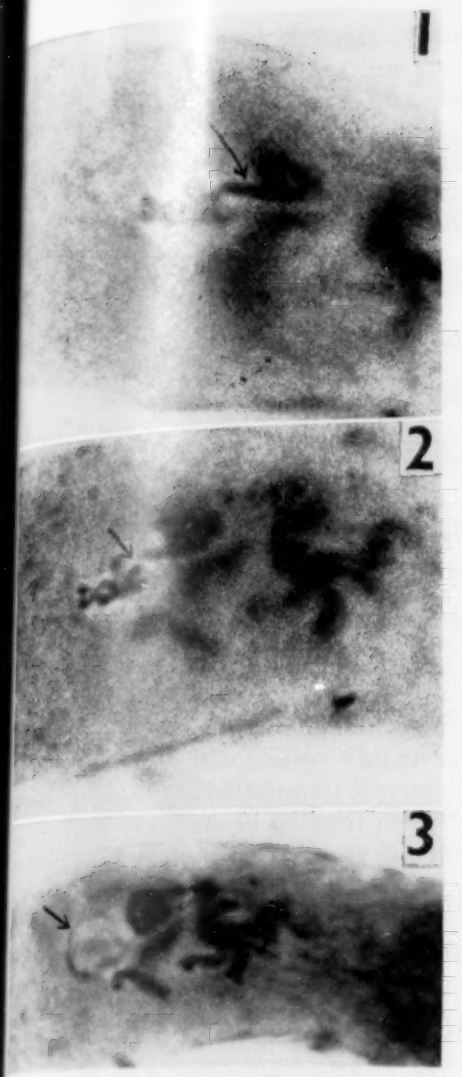

Fig. 1. Part of a basidium showing nucleolar chromosome in diplotene with its long arm (arrow) in full focus and its satellite zone partly in focus. (About × 4040.) Fig. 2. Same as Fig. 1, with short arm in full focus, showing satellite zone and interstitial chiasmata; arrow indicates possible location of centromere. (About × 3760.) Fig. 3. Same as Figs. 1 and 2, showing relative position of the nucleolus (arrow). (About × 1880)

nucleolus has been observed to cover the short arm of the chromosome in its entirety (Fig. 3). It seems likely that the nucleolus is organized by the so-called nucleolar constriction region of the chromosome. Matsuura (*6*) has distinguished two types of nucleolar chromosomes in the higher plants: the interstitial type and the terminal type. The nucleolar chromosome observed in *S. gentilis,* if this distinction is followed, belongs to the interstitial type.

As is well known, the morphological features of this chromosome are better revealed in the plant cells. The fungal nuclei have been considered by some investigators, such as Olive (*7*), to be more or less similar to those of the higher plants. The finding of a nucleolar chromosome with distinctive morphology of its own would appear to substantiate this view.

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4. The abbreviation "SAT" signifies, in addition to "satellite," "*sine acido thymonucleinico*" (without thymonucleic acid); since it has both connotations, the abbreviated form "SAT-zone" is widely used in current cytological literature.
5. I am indebted to the National Institute of Sciences of India for awarding me a research fellowship during the tenure of which this work was performed. Grateful acknowledgments are also due to Dr. S. P. Agharkar for laboratory facilities and encouragement, to Drs. M. J. Thirumalachar and G. B. Deodikar for helpful suggestions, and to Dr. S. H. Tulpule for furnishing advice and literature on cytology.
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20 May 1957

Histochemical Localization of Acid Phosphatase in Bone Tissue

Analysis of the histochemical distribution of an enzyme is a valuable method for understanding and interpreting its physiological role. There is an extensive series of papers on the histochemical distribution of alkaline phosphatase in bone tissue, especially on its distribution during bone formation (*1*), and on the basis of these data and others of a biochemical character, some hypotheses have been put forward with respect to the action of alkaline phosphatase—namely, that its action takes place preponderantly in the cartilaginous calcification and formation of bone matrix and its later calcification.

Biochemical data exist relative to the presence of acid phosphatase in bone tissue; the increase of acid phosphatase in metastasic zones of some carcinomas (carcinoma of the prostate, for example) accompanied by a rise in the level of this enzyme in serum is known (*2*).

Although the histochemical localization of acid phosphatase in diverse tissues and organs has been investigated, because of technical difficulties, its localization in calcified tissues has not been studied in detail.

Recently we made a thorough analysis to establish the basic conditions for making a correct decalcification of bone tissue in order to show the above-mentioned enzyme. On the other hand we proved that the technique for demonstrating this enzyme could not be applied if even a vestige of calcium remained in the slides; chelating agents were tried but with no success.

Following these studies, we started a systematic study of the histochemical distribution of this enzyme in the normal ossification processes in man, rats (stock), and mice (strains C_3H and BAL). Numerous pathological specimens showing osteogenic phenomena and destruction of bone tissue were also used. All the material was treated, after being fixed in neutral 10-percent formalin for 24 hours at 4°C, in a buffer solution of 5-percent formic acid and 20-percent sodium citrate in equal parts, during a preliminary period, until all calcium was eliminated. Later on a modification of Gomori's technique (*3*) and the azo-dye method recently developed by Burton (*4*) were used on the frozen sections. The two techniques gave comparable results insofar as topographic and histological localization was concerned, and the differences in details of secondary importance, of a cytological order, were minimal.

Acid phosphatase was shown to be present in large quantities in the giant cells found in the proximity of erosive bone surfaces (osteoclasts) and cartilaginous surfaces (chondroclasts); we also found large quantities of enzyme in the walls of the vessels adjacent to erosive surfaces. The behavior of the enzyme was similar in the three species studied. In the pathological cases, an association of this enzyme with areas of bone reabsorption was evident, the enzyme being also found in abundance in the multinuclear giant cells of giant cell tumors and other related processes—cells which show a relationship to osteoclasts, even though this be only morphologic (Fig. 1).

In conclusion, we can report that, by using an adequate technique for decalcification, it is possible to show, easily and in a consistent and regular fashion, acid phosphatase in the hard tissues,

Fig. 1. Enchondral ossification zone in the limb of a newborn infant [Gomori's acid phosphatase techniques (frozen section); incubation time, 10 minutes]: (*a*) proliferating cartilage; (*b*) hypertrophic and calcified cartilage; (*c*) bone trabeculae in formation. The most intense enzymatic activity is observed in chondroclastic (*d*) and osteoclastic (*e*) cells. The hypertrophic and calcified zone is lacking in enzyme. (× about 120)

where it is intimately related to the areas of reabsorption; it should therefore be considered that this enzyme plays an important part in this mechanism.

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24 December 1957

Transmission of Internal Cork of Sweet Potato by the Cotton Aphid, Aphis gossypii Glover

Rankin (*1*) reported that *Myzus persicae* (Sulzer) and other aphids are probable vectors of internal cork of sweet potato. However, results from repeated tests with *M. persicae* were not always consistent when roots from test plants were indexed after one season. Hildebrand and Smith (*2*) reported aphid transmission of internal cork; they used various leaf symptoms as criteria of the disease. Sheffield (*3*) reported a disease of sweet potato from East Africa which she designated virus B and suggested its similarity to internal cork of sweet potato in the United States. She reported spread of this disease from sweet potato to sweet potato by the white fly, *Bemisia tabaci* (Genn).

Because *Myzus persicae* is virtually absent in sweet potato fields of Louisiana when much of the infection takes place, an intensive study was begun in 1955 to find a vector responsible for spread of the disease. The prevalence and occurrence of insects associated with sweet potatoes in the mother beds and in the fields was determined in 1956 and 1957. Weekly collections of aphids were made on tanglefoot traps placed in fields 1 foot above the growing vines (*4*). Insects also were collected from vines in the major sweet-potato-growing areas at weekly intervals.

Aphid species coinciding in greatest numbers with plant infection (*5*) were the cotton aphid, *Aphis gossypii* Glover, and the cowpea aphid, *Aphis medicaginis* Koch, other species being relatively few in number. When these two species of aphids were caged on sweet potato leaves, all cowpea aphids perished within 24 hours. The cotton aphids showed signs of discontent, with much probing and moving about. Nevertheless, they managed to survive for 7 to 10 days but did not reproduce. From these and other data it was surmised that the cotton aphid might transmit a virus of a nonpersistent nature in the vector (*6*) from sweet potato to sweet potato.

The following tests were made to determine the possible relationship of certain insect species to transmission of the disease. Eleven insect-proof cages made of 32-by-32-mesh Saran screen were used. Under each cage four cork-free plants (*7*) and one cork-affected plant were set on 9 August 1956. Insects were released into the cages during the first week of September. The species used and the number released per cage are shown in Table 1. In some cages combinations of certain species of insects were used in an attempt to obtain data on a possible complex of viruses. Plant material for 1957 plantings was obtained by cutting vines from the plants in each treatment prior to freeze injury in December 1956. These vines were maintained in the greenhouse until plants were set in June 1957. Some of these plants were grown in the field and were, therefore, subject to infestation by various insect species. The remainder were grown in a 32-by-32-mesh Saran screenhouse free from insects.

Results of these tests (the root-lesion indexing method was used for determining internal cork) are given in Table 1. The data clearly demonstrate that *Aphis gossypii* transmitted an agent, or agents, which resulted in the development of internal cork lesions in the roots of a cork-free stock of the Unit I Porto Rico variety of sweet potato. This was the only species involved which was capable of transmission of the disease under the conditions of this test. Furthermore, the data indicate that virus concentration in the plant must reach a high threshold level before lesions develop extensively in the roots.

Work is continuing to determine whether other viruses and insect species may be involved. It is considered possible that more than one virus may be present before cork lesions appear in the roots.

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Table 1. Data showing transmission of internal cork of sweet potato by the cotton aphid. Roots were examined for cork lesions by slicing into transverse section approximately 1/16 in. thick. For comparative purposes, the mean percentage of roots with lesions from originally cork-affected plants in each cage was determined (20 percent in 1956; 24 percent in 1957).

Insect species used	Grown in 1956			Grown in 1957			
				Uncaged field planting		Caged in screenhouse	
	Cages in 1956 (No.)	Roots examined (No.)	Roots with lesions (%)	Roots examined (No.)	Roots with lesions (%)	Roots examined (No.)	Roots with lesions (%)
50 *Aphis gossypii* + 100 *Empoasca* spp.	1	16	6.3	60	5.0	3	0
50 *Aphis gossypii* + 15 *Draeculacephala* sp.	1	10	0	75	9.3	6	50.0
50 *Aphis gossypii*	3	47	4.3	252	7.5	85	23.5
100 *Empoasca* spp.	2	19	0	123	3.3	29	0
15 *Draeculacephala* sp.	1	13	0	25	0	6	0
100 White flies, unidentified, from *Jacquemontia* sp.	1	16	0	83	2.4	5	0
None—check	2	43	0	190	1.6	26	0

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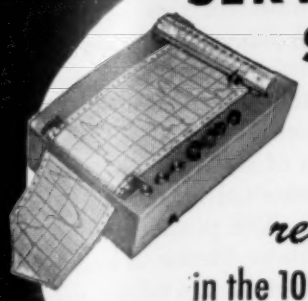

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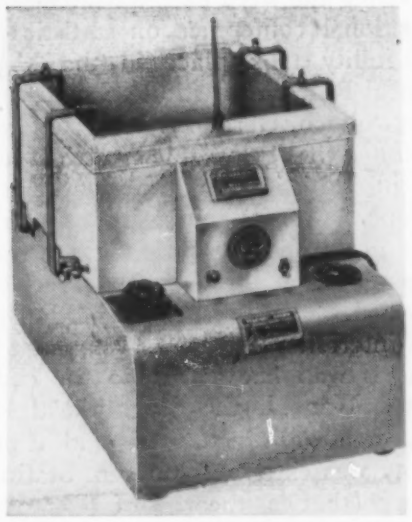

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Meetings

Statistics and Probability

During the week of 2–8 March, an international conference on statistics and probability took place in the Mathematical Research Institute, Lorenzenhof, Oberwolfach, Germany. This conference exhibited the remarkable growth of interest in statistics on the continent of Europe. Also it emphasized a certain shift in emphasis in statistical research in general. For these reasons, a brief account of the conference may be of general interest.

As is well known, up to the end of World War II, the geographical location of research in statistics and in probability was extremely uneven. Statistical theory (that is, theories of tests of hypotheses, of estimation, and of decision functions) was developed mostly in English-speaking countries and was largely ignored in western Europe. Probability theory centered in France and in Russia, while English-speaking countries trailed behind. After the end of the war, this situation changed radically. The works of Bochner, Doob, Feller, and Loève brought this country to the forefront of probabilistic research, and, at the same time, there appeared in Europe several young centers of research in statistics which promise remarkable developments. In addition to this geographical redistribution of effort, the end of the war marked a change in the subject of statistical research which may justify a distinction between what may be called the classical and the modern theories of statistics. I propose to use the term *classical* to describe those sections of statistical theory that deal with *numerical* random variables. They may be exemplified (i) by measurements, subject to random error, of a given physical magnitude; (ii) by responses to a treatment of organisms selected at random from their population; (iii) by pairs of numbers of ions generated by a cosmic ray particle in two adjoining unit lengths of its path; and so on. In each of these cases we deal with a chance mechanism producing *numbers,* either singly or in pairs or in multiple groups. More recently, modern science and modern industry brought under consideration more complicated chance mechanisms, connected with the term *stochastic processes,* which, at a single trial, produce not just a single number or a finite set of numbers but more complicated mathematical entities, such as a function. In the classification proposed, the term *modern theory of statistics* is applied to the statistical theory relating to stochastic processes—that is, to random variables whose possible values are not numbers but functions.

The basis for this subdivision of statistical problems into the categories "classical" and "modern" is the difference in the mathematical apparatus needed for their treatment. Consider a classical problem of statistics with some n observable numerical random variables $X_1, X_2, \ldots, X_n$. The possible values of each X_i are numbers. Hence, if one visualizes the n-dimensional Euclidean space, a single observation on these variables can be represented by a single "point" with its first coordinate equal to X_1, its second coordinate equal to X_2, and so on. The probability that the random point $(X_1, X_2, \ldots, X_n)$ will fall within any specified region, say R, is then represented by the measure of this region. The theory of measure is based on the works of Borel and Lebesgue, developed early in this century and now commonly taught in all universities with reasonably developed mathematical programs. The situation is different with modern statistical problems in which the outcome of a single "trial" is a curve, say $f(x)$ for values of x between zero and unity. If one tries to extend to this case the representation of the outcome of a trial by a point, it is necessary to deal, roughly speaking, with as many coordinates as there are numbers between zero and unity—that is, the continuum. For "spaces" filled with points of this kind ("abstract spaces"), one needs a definition of "distance," of "measure," and so forth. Theories of these concepts (Hilbert spaces, Banach spaces, and so on) are of more recent origin and are included in our university programs only in the most advanced institutions.

As far as "modern" problems are concerned, it was logical and unavoidable that their probabilistic treatment should precede their statistical treatment. The priority here belongs to the Russian school of Kolmogorov and Khintchine. More recently, the remarkable book by Doob clarified many concepts and cleared new paths. While it is difficult to point out exactly the first attempts at statistical problems concerned with stochastic processes, we certainly owe a great deal to the Scandinavian school of Cramér, particularly to Grenander. However, modern statistical theory is still in its infancy. One reason for this state of affairs is the tendency of statisticians to concern themselves with the so-called "practical" problems suggested by various fields of application, and, until recently, both science and industry offered only problems with numerical random variables. However, currently one encounters a number of problems where the observable random variables are curves. One example must suffice. The "observations" by a radio telescope provide a single oscillating curve summariz-

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ing the radio emissions of all the sources located within a certain solid angle. Cosmological theories are concerned with the spatial distribution of these sources (one particular chance mechanism) and with the distribution of their individual intensities (another chance mechanism). Also, the records of emission of the cosmic radio sources are combined by the telescope with unavoidable "noise" (a third chance mechanism). The statistical problem here is to use the observable random curve in order to verify this or that cosmological theory. Modern industry provides similar problems. Thus, the domain of probability distributions in abstract spaces that, until recently, was frequently considered one of the dreams of abstract-minded pure mathematicians, is now rapidly becoming a domain of practical statistical problems. In order to stand up to these practical demands, the efforts of theoretical statisticians must be reoriented.

The conference in Lorenzenhof reflected both of the changes indicated: the change in the geographical distribution of effort in statistical research and the change from classical to modern statistical problems. Of the 42 participants, 32 were from Germany and ten were from other countries (three from the United States and one each from Austria, Czechoslovakia, Egypt, Finland, France, Holland, and Hungary). Each of the foreign visitors and nine Germans were asked to present papers. The contents of these papers are far too technical to be reported here, and the following brief enumeration is limited to cases where the contributions at the conference indicated the emergence of a new statistical research center on the continent of Europe. There appear to be at least five such centers, each inspired by an outstanding personality. Roughly, from West to East, these centers are Paris, Hamburg, Munich, Prague, and Budapest.

For quite some time Paris has been one of the foremost centers of research in probability. However, the theory of statistics seemed to attract less attention. Now this situation has changed, and the conferences at Lorenzenhof were much impressed by a very interesting statistical paper by Robert Fortet, a noted probabilist. This paper dealt with the theory of testing statistical hypotheses concerned with stochastic processes and established an intriguing connection with Shannon's theory of information.

Hamburg was represented by L. Schmetterer, leader of the group; by the recently appointed docent, Krickeberg; and by a group of promising graduate students. Incidentally, Schmetterer was responsible for the organization of the scientific program of the conference at Lorenzenhof. His own paper was probabilistic and dealt with limit theorems on groups. Krickeberg spoke on convergence

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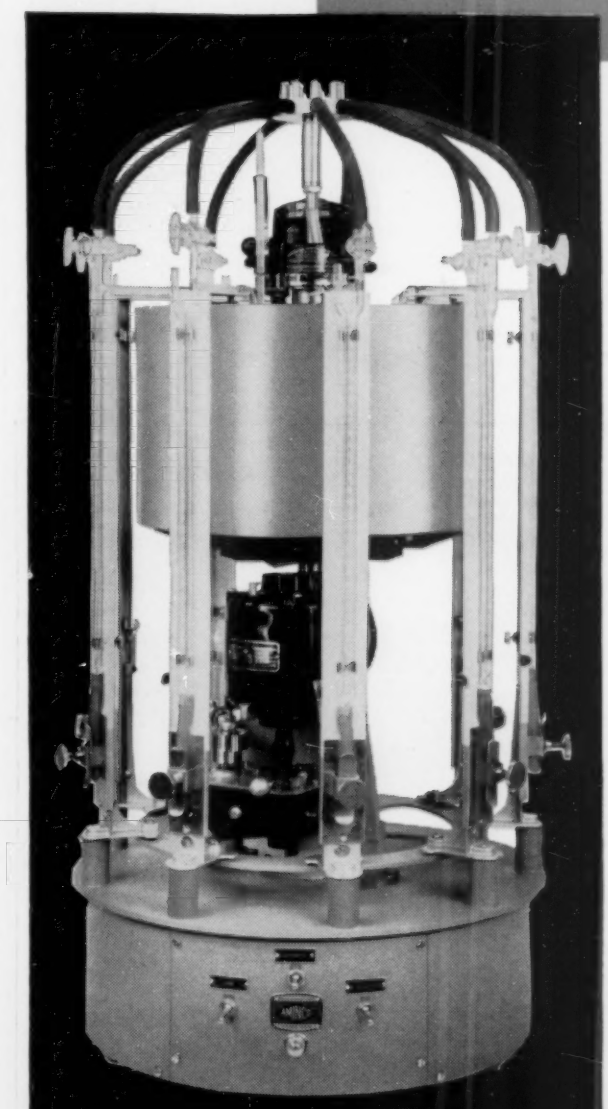

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of stochastic processes and exhibited signs of the influence of Doob.

The center of Munich provided four active participants, D. Bierlein, K. Jakobs, K. Gaede, and H. Richter. The first three spoke on problems of probability, while Richter dealt with Lindley's version of the statistical decision problem. The same problem was also treated by E. Brandau of Tübingen.

The two statistical groups of eastern Europe, one in Prague and the other in Budapest, were represented at the conference by their respective leaders, A. Spacek and A. Renyi. Their papers testified to the very intense and modern activity of the two groups and were loaded wtih material.

While several of the papers mentioned, and also some of those omitted from the above enumeration, were essentially probabilistic in character, they included distinct statistical elements. Also, predominantly, the papers presented dealt with modern problems.

Taking into account the activities of the new centers just described, together with those of the older centers in Holland, Denmark, Scandinavia, and Poland, all subject to a considerable extent to influences of the Russian school, one obtains the impression of a very strong drive now prevalent in Europe towards the development of an important new chapter of statistical theory.

The present account would be incomplete without a few lines about the Mathematical Research Institute at Lorenzenhof. Erected in the hills of Schwartzwald early in this century, the Lorenzenhof was intended to serve as the hunting lodge of a magnate. In due course it became the property of the state and, in 1944, became the seat of the Mathematical Research Institute. Originally, the intention was to concentrate certain branches of war research in the institute. However, in practice, the Lorenzenhof became a refuge for some of the German mathematicians who, for various reasons, had to abandon their university positions. Currently, under the wise guidance of its director, W. Süss of Freiburg University, and of his colleague and friend, H. Kneser of Tübingen University, the activities of the institute consist of the organization of some 12 mathematical symposia per year. Each symposium is given to a separate mathematical discipline and lasts about a week. From time to time, individual mathematicians come to Lorenzenhof for a period of quiet work.

As a means for fostering research, the usefulness of the institute and of its symposia is supreme. Leisurely life in beautiful surroundings, divided between a moderate number of lectures, walks in the countryside, and occasional periods of good music, creates excellent opportunities for informal contacts and discussions which have a most beneficial influence on the development of mathematicians in Germany and, through a substantial attendance from abroad, also elsewhere. One cannot help asking: What about establishing a similar institution in the United States?

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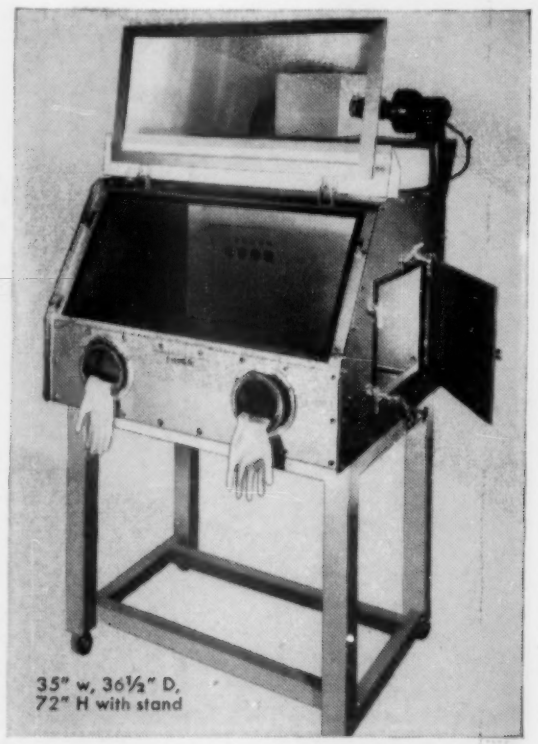

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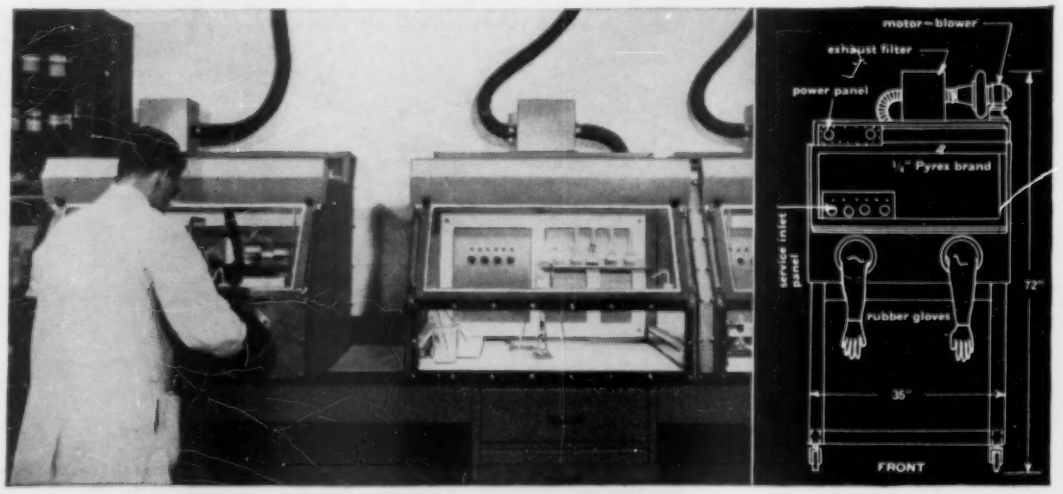

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First All-India Conference of Zoology

The Zoological Society of India is organizing the first All-India Congress of Zoology, to be held in Calcutta from 31 October to 6 November. This is the first time that an attempt has been made to bring together all the zoologists in India. A number of delegates from foreign countries are also expected to attend. Dr. B. S. Chauhan of the Zoological Survey of India, Calcutta, is the local secretary. The last date for submission of papers to be read before the congress is *15 August*.

Society Elections

▪ Oregon Academy of Science: pres., C. R. Monk, Willamette University; v. pres., Lloyd W. Staples, University of Oregon; past pres., E. C. Gilbert, Oregon State College; sec., and representative to the AAAS Council, F. A. Gilfillan, Oregon State College, Corvallis, Ore.; treas., E. A. Yunker, Oregon State College.

▪ National Association for Research in Science Teaching: pres., Thomas P. Fraser, Department of Science Education, Morgan State College, Baltimore, Md.; v. pres., Vaden W. Miles, Physics Department, Wayne State University; sec.-treas., Clarence M. Pruitt, University of Tampa, Tampa, Fla. The representative to the AAAS Council is George G. Mallinson.

▪ American Society of Ichthyologists and Herpetologists: pres., Edward H. Taylor; treas., James E. Bohlke, Academy of Natural Sciences, Philadelphia, Pa.; sec., Roger Conant, Philadelphia Zoo, 34th St. and Girard Ave., Philadelphia 4, Pa. The vice presidents are: Boyd W. Walker, Department of Zoology, University of California; John C. Marr, La Jolla, Calif.; James A. Kezer, Department of Biology, University of Oregon.

Forthcoming Events

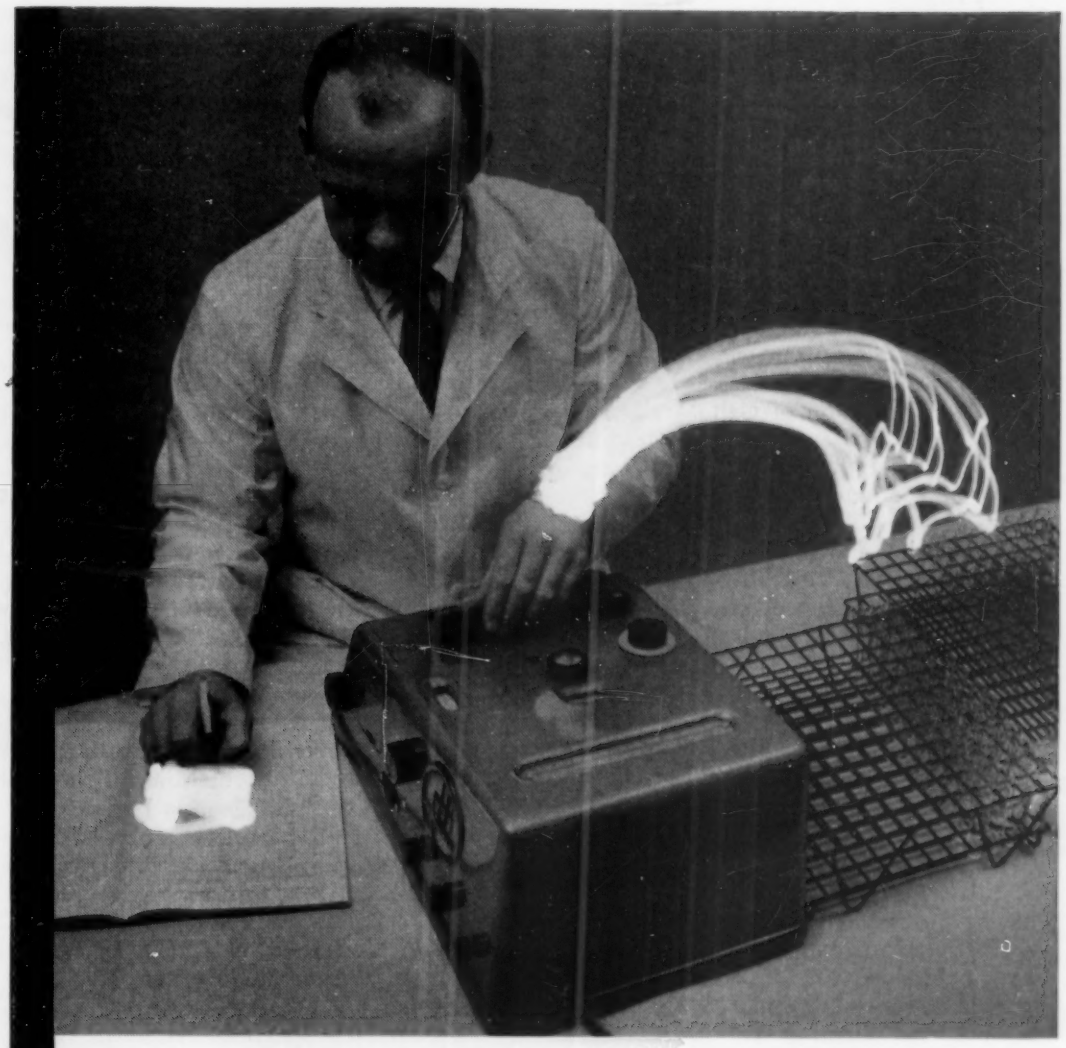
July

20–23. National Shellfisheries Assoc., 49th annual, Baltimore, Md. (P. A. Butler, U.S. Shellfisheries Laboratory, Gulf Breeze, Fla.)

20–27. Americanists, 33rd intern. cong., San Jose, Costa Rica. (33rd Intern. Cong. of Americanists, National Museum, P.O. Box 749, San Jose de Costa Rica, Central America.)

21–24. High Polymer Conf., intern., Nottingham, England. (Conference Secretariat, Dept. of Scientific and Industrial Research, Charles House, 5–11, Regent St., London, S.W.1.)

22–26. Brazilian Soc. for the Progress of Science, 10th annual, São Paulo, Brazil. Sociedade Brasileira para o Progresso da



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23–28. Continuous Cultivation of Microorganisms Symp. (by invitation), Prague, Czechoslovakia. (I. Malek, Inst. of Biology, Czechoslovak Akad. of Sciences, Narodni Tr. 5, Prague I.)

24–25. Computers and Data Processing, 5th annual symp., Denver, Col. (Electronics Div., Denver Research Inst., Univ. of Denver, Denver 10.)

25–29. Chromatic Discrimination in Animals and Man, ICSU symp., Paris, France. (H. Pieron, Collège de France, Place Marcelin-Berthelot, Paris 5e.)

28–30. Regulation of Cell Metabolism, Ciba Foundation symp. (by invitation), London, England. (G. E. W. Wolstenholme, 41 Portland Pl., London, W.1.)

28–2. Home Economics, 9th intern. cong., College Park, Md. (Congress Director, American Home Economics Assoc., 1600 20 St., NW, Washington 9.)

28–8. Statistical Summer Seminar, Dedham, Mass. (I. Weiss, Bell Telephone Labs., North Andover, Mass.)

August

4–9. Microbiology, 7th intern. cong., Stockholm, Sweden. (F. C. Harwood, Soc. of American Bacteriologists, c/o Waverly Press, Inc., Mt. Royal and Guilford Aves., Baltimore 2, Md.)

7–9. Electron Microscope Soc., annual, Los Angeles, Calif. (C. M. Schwartz, Battelle Memorial Inst., 505 King Ave., Columbus 1, Ohio.)

10–16. Radiation Research, intern. cong., Burlington, Vt. (H. M. Patt, Argonne National Lab., P.O. Box 299, Lemont, Ill.)

11–13. International Mathematical Union, 3rd general assembly, St. Andrews, Scotland. (F. Smithies, Mathematical Inst., 16 Chambers St., Edinburgh 1, Scotland.)

11–16. Occupational Therapists, World Federation's 2nd intern. cong., Copenhagen, Denmark. (Mrs. I. Worsoe, Hvidkloververj 10, Aarhus, Denmark.)

12–13. Economic Botany Conf., New York, N.Y. (D. J. Rogers, New York Botanical Garden, Bronx Park, New York 58.)

13–15. Electronic Standards and Measurements Conf., Boulder, Colo. (J. F. Brockman, National Bureau of Standards, Boulder.)

13–15. Industrial Applications of X-ray Analysis, 7th annual conf., Denver, Colo. (W. M. Mueller, Metallurgy Div., Denver Research Inst., University of Denver, Denver 10.)

13–19. Seaweed Symposium, 3rd intern., Galway, Ireland. (C. O. hEocha, Chemistry, Department, Univeristy College, Galway.)

13–20. Insect Pathology and Biological Control, intern. conf., Prague and Smolenica, Czechoslovakia. (J. Weiser, Inst. of Biology, Nacvicisti 2, Prague XIX, Czechoslovakia.)

13–20. International Astronomical Union, 10th general assembly, Moscow, U.S.S.R. (P. Th. Oosterhoff, IAU, Leiden Observatory, Leiden, Netherlands.)

15–20. World Medical Assoc., 12th general, Copenhagen, Denmark. (World Medical Assoc., 10 Columbus Circle, New York 19.)

17. American College of Hospital Administrators, 24th annual, Chicago, Ill. (ACHA, 620 N. Michigan Ave., Chicago 11.)

17–21. Health Conf., 7th annual, University Park, Pa. (M. Cashman, Pennsylvania Dept. of Health, P.O. Box 90, Harrisburg.)

18–19. American Astronautical Soc., Western meeting, Palo Alto, Calif. (N. V. Petersen, Lockheed Missile Systems Div., Palo Alto.)

18–21. Conservation Education Assoc., 5th annual, Salt Lake City, Utah. (S. D. Mulaik, Biology Dept., University of Utah, Salt Lake City.)

18–21. Heat Transfer, AIChE conf., Evanston, Ill. (F. J. Van Antwerpen, AIChE, 25 W. 45 St., New York 36.)

18–22. Clinical Chemistry Workshop, Houston, Tex. (Division of Clinical Chemistry, Dept. of Biochemistry, Baylor Univ., College of Medicine, Houston. 25.)

18–22. Occupational Medicine and Toxicology, 2nd Inter-American conf., Miami, Fla. (W. B. Deichmann, Dept. of Pharmacology, Univ. of Miami School of Medicine, Coral Gables, Fla.)

18–22. Plant Science Seminar, 35th annual, Big Rapids, Mich. (E. P. Claus, Div. of Pharmacy, Ferris Inst., Big Rapids.)

18–22. Semiconductors, intern. conf., IUPAP, Rochester, N.Y. (D. L. Dexter, Dept. of Physics, Univ. of Rochester, Rochester.)

18–23. New England Assoc. of Chemistry Teachers, 20th summer, Kingston, R.I. (J. A. Martus, College of the Holy Cross, Worcester 10, Mass.)

18–25. Religion in the Age of Science, 5th summer conf., Star Island, N.H. (Institute on Religion in an Age of Science, 280 Newton St., Brookline 46, Mass.)

20–23. Photofluorography, intern. cong., Stockholm, Sweden. (International Cong. of Photofluorography, P.O. Box 5097, Stockholm 5.)

20–27. Australian and New Zealand Assoc. for the Advancement of Science, 33rd cong., Adelaide, Australia. (J. R. A. McMillan, Science House, 157–161 Gloucester St., Sydney.)

20–27. Genetics, 10th intern. cong., Montreal, Canada. (J. W. Boyes, Dept. of Genetics, McGill Univ., Montreal, P.Q.)

21–23. American Farm Economic Assoc., Winnipeg, Canada. (L. S. Hardin, Dept. of Agricultural Economics, Purdue Univ., Lafayette, Ind.)

21–23. Chemical Organization of Cells, Normal and Abnormal, Madison, Wis. (J. F. A. McManus, Dept. of Pathology, Univ. of Alabama Medical Center, Birmingham.)

21–24. Cenozoic of Western Montana, field conf., Missoula, Mont. (A. E. Wood, Soc. of Vertebrate Paleontology, Dept. of Biology, Amherst College, Amherst, Mass.)

23–25. Rural Sociology Soc., annual, Pullman, Wash. (H. F. Lionberger, Dept. of Rural Sociology, Univ. of Missouri, Columbia.)

24–28. American Inst. of Biological Sciences, annual, Bloomington, Ind. (H. T. Cox, AIBS, 2000 P St., NW, Washington 6.)

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American Bryological Soc., annual. (Mrs. V. S. Bryan, Botany Dept., Duke Univ., Durham, N.C.)

American Fern Soc., annual. (Miss M. E. Faust, 501 University Pl., Syracuse 10, N.Y.)

American Microscopical Soc., annual. (D. G. Frey, Dept. of Zoology, Indiana Univ., Bloomington.)

American Phytopathological Soc., 50th anniversary. (W. B. Hewitt, Dept. of Plant Pathology, Univ. of California, Davis.)

American Soc. for Horticultural Science, annual. (R. E. Marshall, Dept. of Horticulture, Michigan State Univ., East Lansing.)

American Soc. of Ichthyologists and Herpetologists, annual. (R. Conant, Philadelphia Zoological Garden, 34th and Girard Ave., Philadelphia 4, Pa.)

American Soc. of Limnology and Oceanography. (B. H. Ketchum, Woods Hole Oceanographic Inst., Woods Hole, Mass.)

American Soc. of Naturalists. (B. Wallace, Long Island Biological Assoc., Cold Spring Harbor, N.Y.)

American Soc. of Parasitologists, annual. (P. E. Thompson, Research Div., Parke, Davis & Co., Detroit 32, Mich.)

American Soc. of Plant Physiologists, annual. (G. R. Noggle, Dept. of Botany, Univ. of Florida, Gainesville.)

American Soc. of Plant Taxonomists. (R. F. Thorne, Botany Dept., State Univ. of Iowa, Iowa City.)

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Botanical Soc. of America, annual. (H. C. Bold, Dept. of Botany, Univ. of Texas, Austin 12.)

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Mycological Soc. of America, annual. (E. S. Beneke, Dept. of Botany and Plant Pathology, Michigan State Univ., East Lansing.)

National Assoc. of Biology Teachers. (P. Fordyce, Broad Ripple High School, Indianapolis, Ind.)

Nature Conservancy. (G. B. Fell, 4200 22 St., NE, Washington 18.)

Phycological Soc. of America, annual. (W. A. Daily, Dept. of Botany, Butler Univ., Indianapolis 7, Ind.)

Potato Assoc. of America, annual. (R. V. Akeley, Crops Research Div., USDA, Plant Industry Station, Beltsville, Md.)

Society for Industrial Microbiology, annual. (C. L. Porter, Dept. of Biological Sciences, Purdue Univ., West Lafayette, Ind.)

Society of Protozoologists, annual. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana.)

Society for the Study of Development and Growth. (R. O. Erickson, Dept. of Botany, Univ. of Pennsylvania, Philadelphia 4.)

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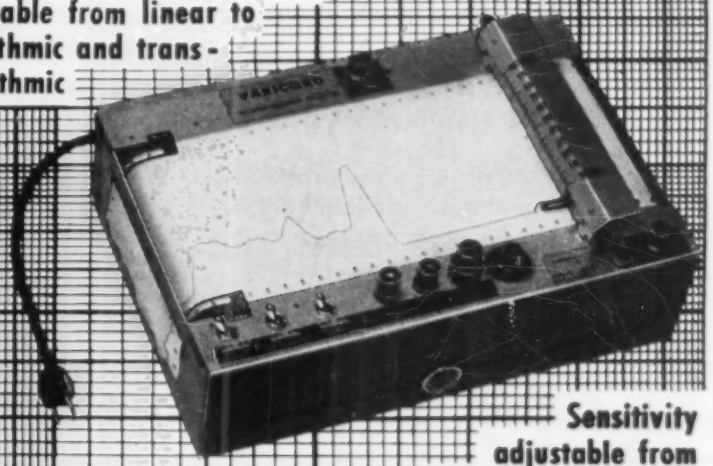

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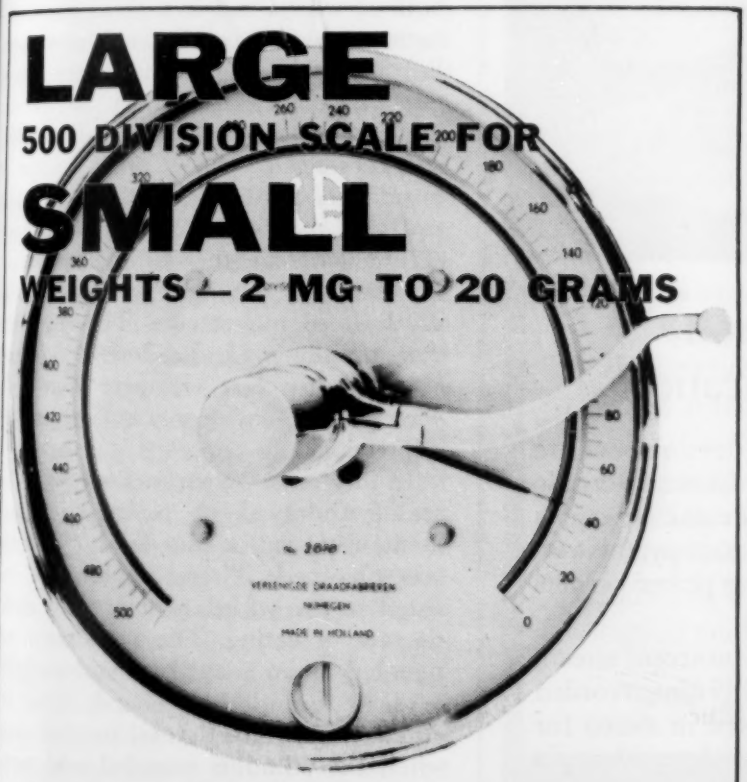

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24–29. Atmospheric Diffusion and Air Pollution, intern. symp., Oxford, England. (F. N. Frenkiel, Applied Physics Lab., Johns Hopkins Univ., Silver Spring, Md.)

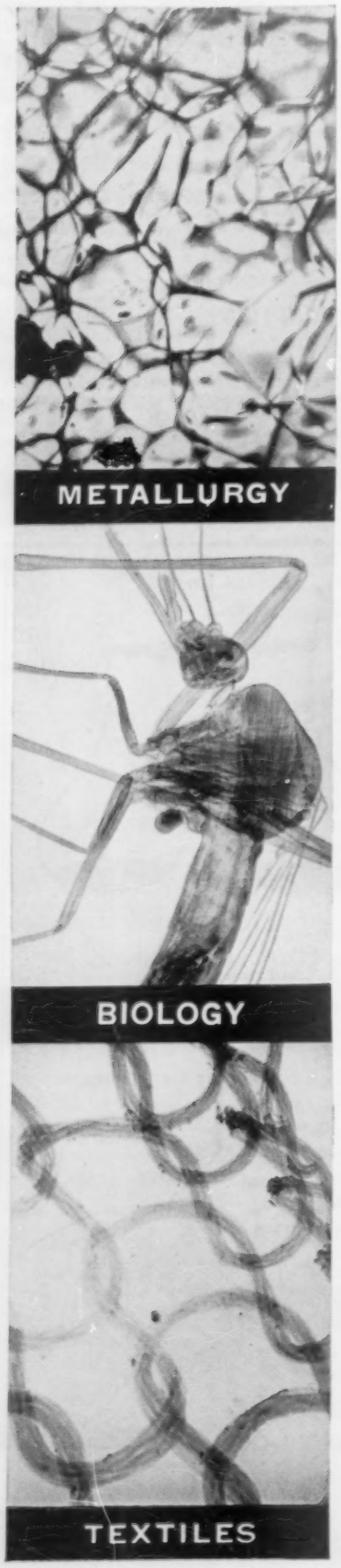
24–29. Mental Health, world federation, 11th annual, Vienna, Austria. (Miss E. M. Thornton, World Federation for Mental Health, 19 Manchester St., London, W.1, England.)

24–30. Prehistoric and Protohistoric Science, 5th intern. cong., Hamburg, Germany. (Büro des Internationalen Kongresses für Vor- und Frügeschichte, c/o Fremdenverkehrs- und Kongresszentrale, Hamburg 1, Bieberhaus, Hachmannplatz.)

25–28. Institute of Mathematical Statistics, annual, Cambridge, Mass. (G. E. Nicholson, Jr., Dept. of Statistics, Univ. of North Carolina, Chapel Hill.)

25–28. Mathematical Assoc. of America, 39th summer, Cambridge, Mass. (H. M. Gehman, Univ. of Buffalo, Buffalo 14, N.Y.)

Erratum: The correct address for the Society for the Scientific Study of Sex is 1 E. 42 St., New York 17, N.Y. The society will hold its first meeting on 8 November at the Barbizon-Plaza Hotel in New York.



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Letters

Withdrawal versus Withholding of Positive Reinforcement

C. B. Ferster (*1*) in his experiments used withholding of food reinforcement (called by him withdrawal of positive reinforcement) in differentiation of conditioned motor reflexes. It seems, however, that under the circumstances of his experiments, the procedure cannot be considered as "the withdrawal of positive reinforcement" nor as "the withdrawal of the situation in which the reinforcement occurs." The situation in which the reinforcement occurred was only partially changed by switching off of the overhead lamp, or by appearance of the red light, and in spite of the fact that the animal performed the movement (pressing of the key), the reinforcement was withheld. The differentiation which was attempted in these experiments developed slowly and was only a partial one, because the reinforcement was withheld regularly only when the movement occurred during periods when the red light appeared, whereas in the absence of the red light the reinforcement was applied irregularly.

Ferster emphasizes in his report the analogy between the punishment and the withholding of positive reinforcement, both of which had as a result the elimination of the conditioned movement. But I think that there is a very important difference between the two events: the punishment, whether used in classic conditioning (type I) or in escape or avoidance conditioning (type II) (*2*), can give rise to new conditioned movements, whereas the differential inhibition can only eliminate some preexisting conditioned movements. The suggestion that punishment and differentiation have common "aversive" features is, as of the present time, based only on introspective impressions.

In my own experiments (*3*), when real withdrawal of positive reinforcement was used, a new kind of behavior was obtained. These experiments consisted in the withdrawal of food during the act of eating. The withdrawal was signaled by an acoustic stimulus applied 5 to 10 seconds beforehand. After several such trials the animal stopped eating when the stimulus sounded and turned away from the food tray. This conditioned "cessation reflex" was later differentiated: the food was withdrawn after one of the stimuli, but after another it was not. During the action of the first stimulus the dog turned away from the food, but during the action of the other (that which was not reinforced by withdrawal of food), he ate without interruptions.

Thus, both the inhibition produced by

withdrawal of food and punishment by pain reinforcement may give rise to new conditioned movements.

A. W. ZBROZYNA

Department of Neurophysiology, Nencki Institute of Experimental Biology, Warsaw, Poland

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3. A. W. Zbrozyna, *Acta Physiol. Polon.* **3**, 70 (1952); *Bull. acad. polon. Sci. Classe VI*, **5**, 261 (1957); *Acta Biol. Exptl. Warszawa*, in press.

A. Zbrozyna is correct in stating that in establishing the aversive event in my experiment I did not withdraw all of the stimuli correlated with reinforcement. The stimulus I withdrew is the one differentially correlated with reinforcement. The conditioned response was *never* reinforced in the absence of the overhead light, while, alternately, it was intermittently reinforced in its presence. That the rate of pressing the key fell to zero in the absence of the overhead light is evidence of its effectiveness. The aversive nature of the absence of the overhead light is demonstrated by the suppression of key pressing during a second stimulus preceding the termination of the experiment (warning stimulus).

This experimental procedure has been studied extensively with electric shock as the aversive stimulus (see *1*). Whether the degree of aversiveness of the stimulus correlated with the nonreinforcement of the conditioned response depends on how many of the stimuli present during reinforcement are withdrawn is an experimental question worth raising. For example, would the discontinuation of the reinforcement of key pressing be more aversive if we physically removed the animal from the experiment for 60 minutes rather than "prevented" him from emitting the conditioned response by presenting a stimulus in whose presence the conditioned response has consistently gone unreinforced in the past?

The paradigm used in my experiment investigates only one aspect of an aversive stimulus: the suppression of some on-going, positively reinforced behavior by a stimulus preceding the aversive event. Other properties of aversive stimuli (such as electric shock) not studied in this experiment include (i) the property of maintaining another response which terminates, postpones, or avoids the aversive event; (ii) the property of differentially suppressing conditioned or otherwise maintained behavior by punishment—that is, by application of the aversive event to the response that is to be suppressed. Experiments demonstrating both the first (*2*, *3*) and second property (*2*) of the discontinuation of

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positive reinforcement have already been carried out, although they have not yet been published.

C. B. FERSTER
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References

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2. C. B. Ferster, *Psychol. Monographs*, in press.
3. R. J. Herrnstein and W. H. Morse, *Am. Psychologist Abstr.* (1956).

Prepublication Problems

The editorial in *Science* [**127**, 623 (1958)] on "Pitfalls of prepublication" called attention to a new type of scientific publication problem.

Problems of printing the works of productive, perhaps overproductive, scholars are old ones. By the 17th century, for example, the practice of rushing into print was deplored by William Harvey, who wrote of "the crowd of foolish scribblers whose observations were as inconsequential as their theories were wordy" (*1*). Lilienthal, in his *De Machiavellismo Literario*, likened the offspring of such scholarly productivity to blind whelps brought forth without pain (*2*). Johann Mencken, writing in 1715 in *De Charlataneria Eruditorum* (*3*), could not overlook mentioning "those writers who consider themselves suitably blessed if no year, or better, no month passes without receiving something new from their exceedingly fruitful minds."

Until I read the *Science* editorial, however, I had been aware of only one complaint regarding prepublication productivity. That complaint concerned Paracelsus, who dictated the majority of his books. One of his students complained that they were dictated at such a speed "you'd think that the devil was speaking in him" (*4*). This prepublication complaint is interesting historically but barely applicable, because Paracelsus' books were handwritten manuscripts.

Today, however, all sorts of duplicating processes exist, making possible an extensive, but strictly informal, kind of publication—that is, prepublication. The *Science* editorial mentioned one reason for prepublication: accelerating the research process. Sending mimeographed copies of articles in press to colleagues makes them immediately cognizant of information that may not appear for months or, in the case of some journals in my own field, for years. In the field of psychology, three other reasons for duplicated copies have been advanced: (i) There is a growing tendency for convention "handouts" to take the form of full drafts of the paper to be read (*5*). (ii) Brief reports, limited to one printed page, are solicited by one journal for early publication. An author, however, is

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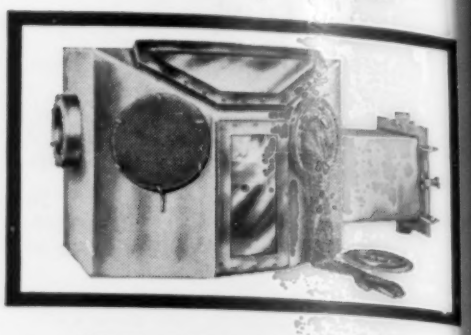

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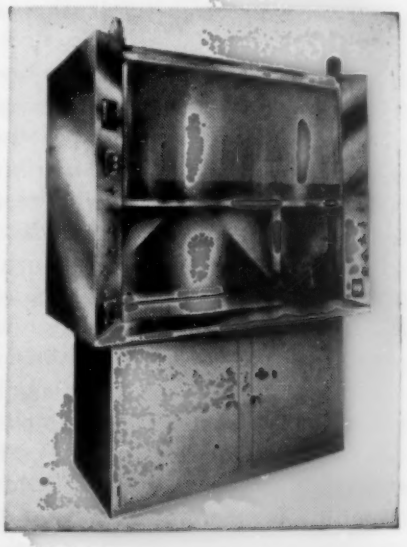

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required to prepare at least 100 mimeographed copies of a full report of the research study to send without charge to all who request it (6). (iii) Authors of manuscripts not yet submitted for publication are routinely advised to give them a "trial run" on professional colleagues [7]. In many cases the feeling seems to be "the more the merrier," and mimeographed copies are scattered broadside. A few years' collection of such items may result in confusion about citations and reduce the helpfulness of the reference section of articles. Gradually, a fixed procedure is being introduced in psychological writing (7). For example, only articles accepted for publication may be designated "in press." In such cases the name of the accepting journal is part of the citation form. If a paper has been presented at a meeting, the preferred forms of citation are (in order of rank) to the published version of the paper, to the published abstract, or (if it is essential to cite the paper and no version exists in the professional literature), to the title and author, followed by a blanket citation, such as "paper read at Va. Acad. Sci., Old Point Comfort, May, 1957." It should be noticed that this least preferred method eliminates citation of a specific page reference for any quotation and thus avoids difficulties occasioned by editorial changes in a version published later. To differentiate between convention "give-aways," which often bear only a title and the author's name for identification, and the full reports mentioned in (ii), I suggested that such material carry a reference to the brief, published report. My suggestion was adopted by the editor, L. F. Shaffer (personal communication). It is in the hope that these methods will be of use to research workers in other fields that they are presented here.

Early alchemists, like Paracelsus, resembled donkeys lured along by carrots dangling before their noses. Today's scientists are like jet planes—propelled by their own exhaust. For, apparently, it is only in our communication-conscious era that problems of prepublication arise. Concrete remedies are necessary, lest the exhaust eradicate bibliographic accuracy.

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